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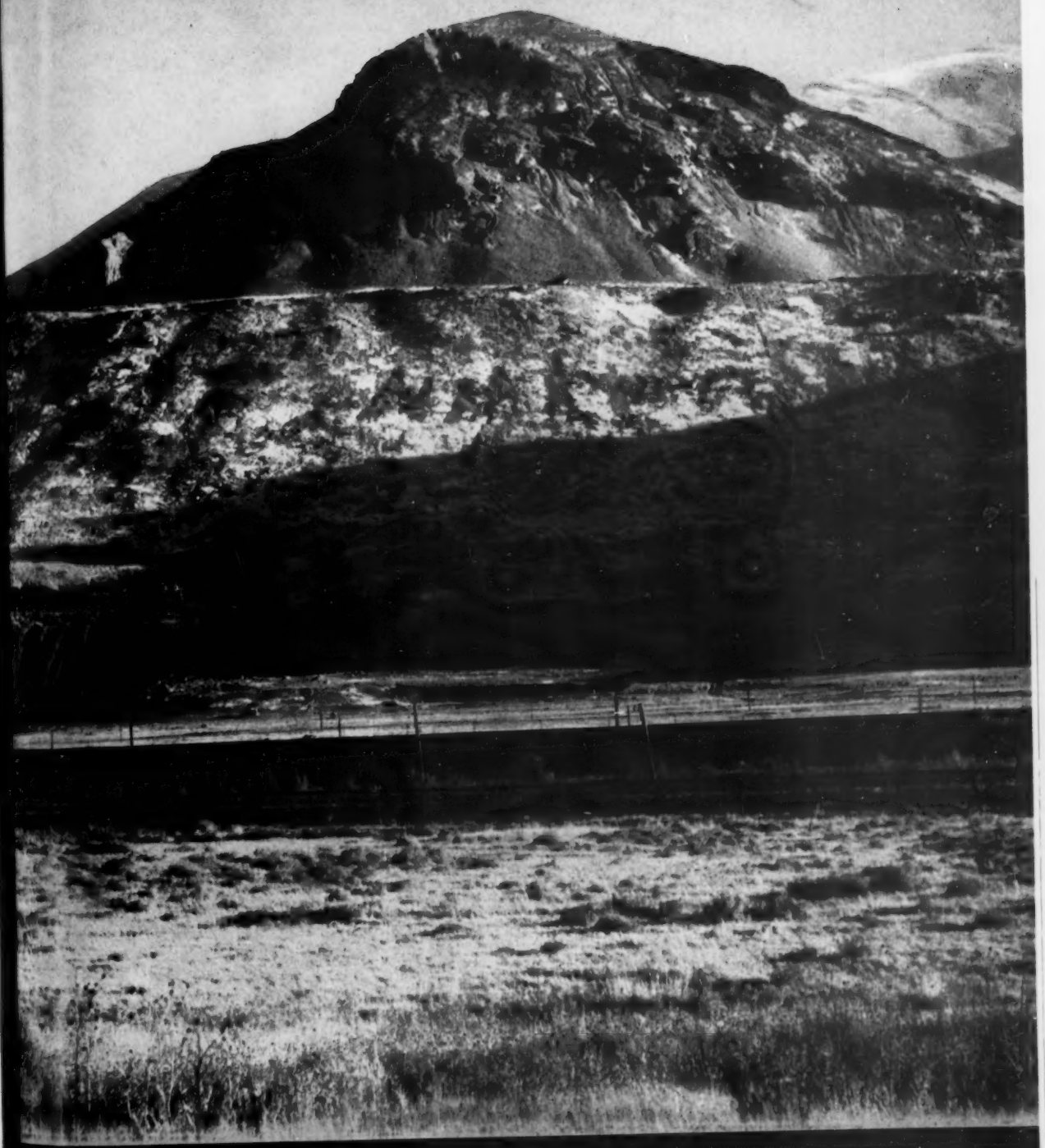

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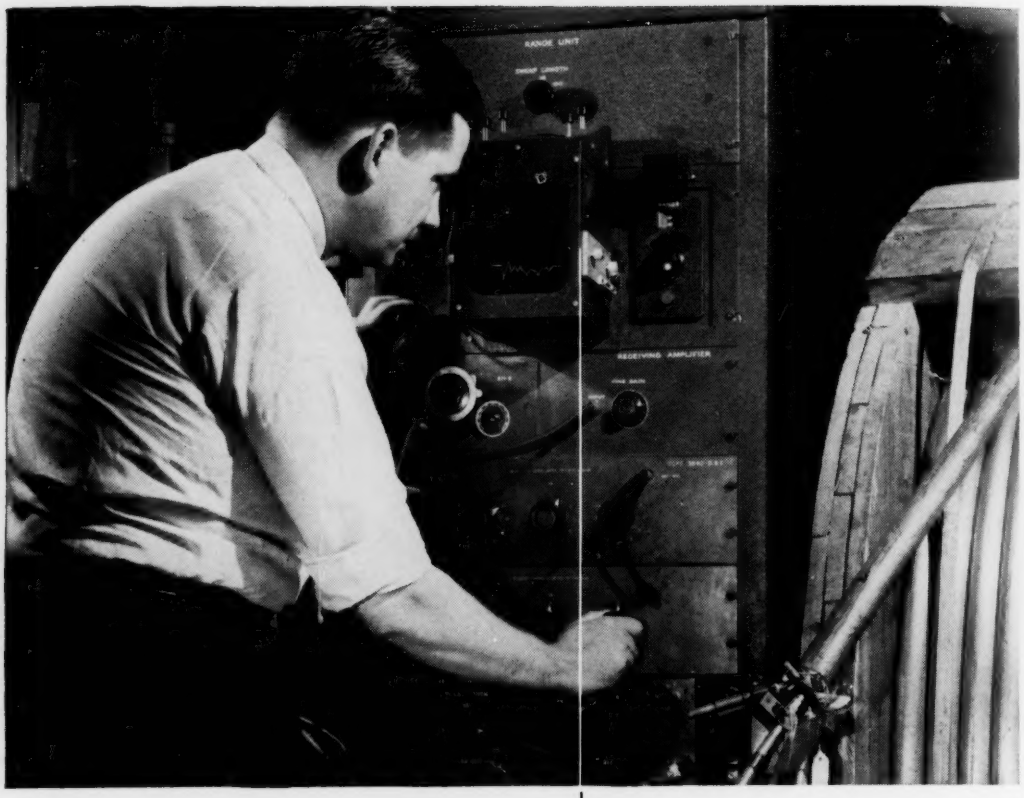

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THE
SCIENTIFIC MONTHLY

DECEMBER 1948

METEORITES, RELATIVE ABUNDANCES, AND PLANET STRUCTURES*

HARRISON BROWN

Dr. Brown (Ph.D., Johns Hopkins, 1941) is associate professor in the Institute for Nuclear Studies at the University of Chicago. During 1943–46 he was assistant director of chemistry for the Clinton Laboratories, Oak Ridge, Tennessee. Dr. Brown was awarded the Annual Thousand Dollar AAAS Prize at the Chicago Meeting in 1947.

PRESENT evidence indicates that approximately three billion years ago the elements were formed, roughly in the proportions in which they exist today. Whatever the process might have been that terminated in the formation of elements, it was followed (probably rapidly) by a process of agglomeration that led to the formation of the bodies of matter which now make up our visible universe: the stars and the planets.

For many years scientists have puzzled over the nature of the processes resulting in the formation of elements, stars, and planets, but most questions concerning the origins of our universe are still either unanswered or at best incompletely answered. The scientist asks on the one hand: What were the conditions that existed in the universe when the elements were formed? What was the process of formation? What are the relationships between the relative amounts in which the elements were formed and the stability of the constituent nuclear species? When did the formation process take place? On the other hand he asks: What processes were involved in the agglomeration of matter, once formed, into stars? When and how were the planets of our solar system formed? What are the internal structures of the planets?

Essentially all branches of the physical sciences have been utilized in attempts to answer these profound questions. Astronomy, the various branches of physics (including nuclear physics), geology, and chemistry—each of these sciences has brought specialized knowledge to bear upon the problems. Seldom is the scientist confronted with major problems involving such large numbers of the physical sciences.

More recently, in large measure due to the classic work of such men as Farrington, Merrill, Prior, Tammann, Goldschmidt, Jeffreys, and Daly, the science of meteoritics has been used as a tool in solving many of the problems concerned with the origins of things. Work done during the past few years now strongly indicates that in meteoritics scientists possess a Rosetta stone that may well prove to be a major key in answering some of the questions propounded above.

Meteorites and their origin. Meteorites are solid bodies which arrive on the earth's surface from outer space. They range in size from a few milligrams to many tons. If one is seen to fall and is subsequently picked up, the meteorite is called a *fall.* If the meteorite is not seen to fall, but is instead found in the countryside, it is called a *find.* There are many interesting and important features of meteorites which space does not permit discussing here; the reader is referred to a recent popular review by Watson.[1] In the present discussion

* Photographs by courtesy of the Smithsonian Institution, Washington, D. C.

Tulia, Swisher County, Texas, Meteorite. About natural size, photograph shows "flight markings" on the forward face of the meteorite. Composition of this chondrite type is: olivine, 34.71%; enstatite, 25.64%; feldspar, 12.92%; iron and nickel, 4.14%; and troilite, 7.39%.

we shall be interested primarily in features of meteorites that are related to their composition.

Meteorites in general are of two types: *iron* meteorites and *stony* meteorites. Iron meteorites are fragments of pure metal, consisting primarily of an alloy of iron and nickel. Stony meteorites consist primarily of magnesium, calcium, and iron silicates through which finely divided particles of metallic iron-nickel are dispersed. The stones may thus be thought of as consisting of two phases: the *silicate phase* and the *metal phase*. In a pulverized meteorite, the two phases can be separated magnetically. A third phase, known as *troilite* and consisting in the main of ferrous sulfide, exists both in stony and in iron meteorites. A few meteorites are known, termed *pallasites,* which consist of approximately equal proportions of silicate and metal phases.

The average compositions of stony and iron meteorites[2] are given in Table 1, together with the compositions of the silicate phase (with the metal and sulfide phases removed), the metal phase, and the composition of igneous rocks.[3] The latter make up approximately 95 percent of the earth's crust. The averages for stony meteorites are based largely upon falls.

Inspection of Table 1 suffices to show that stony meteorites differ markedly in composition from igneous rocks, the latter being considerably higher in oxygen and silicon contents and lower in magnesium, chemically combined iron, and metal-phase contents. On the other hand, the compositions of iron meteorites and of the metal phase of stony meteorites bear a striking resemblance to each other. Meteorites clearly resemble no known bodies of matter found in large quantity on the surface of the earth. Yet there is a certain similarity in the compositions of igneous rocks and the silicate phase of stony meteorites. The similarity gives rise to the question as to whether there might not be a family relationship between meteoritic and terrestrial matter.

As early as 1850 A. Boissé suggested that the earth's composition is probably equivalent to the mean composition of meteoritic matter. Since that time much effort has been expended by scientists in attempts either to develop or to disprove Boissé's speculation. Investigations during the past fifty years have yielded sufficient evidence to give basis to two beliefs that have been generally held: (1) that the dense core of the earth, as defined by seismic data, is probably similar to iron-nickel meteorites in composition; (2) that the silicate phase of stony meteorites is similar to the mantle surrounding the core of the earth. The logical conclusion to be drawn from these beliefs, if the beliefs can be transformed into fact, is that meteorites are the fragments of a planet, similar to the earth in general chemical characteristics.

Recently thermodynamics has been applied to the problem of the composition of meteoritic matter[2] with considerable success, giving further weight to the hypothesis that meteorites had their origin in a planet. Perhaps the most convincing evidence in this connection is that derived when one applies thermodynamics to the distribution of elements between the meteoritic phases.

It has been recognized for some time that those major meteoritic constituents whose oxides possess low heats of formation exist primarily in the metal phase. This phenomenon has been put upon a quantitative basis,[2] and it has been demonstrated that the observed distribution of elements between the metal, silicate, and sulfide phases of meteorites can be explained satisfactorily if one assumes that the distribution was obtained at a temperature of approximately 2,000° K and a total pressure of about 1.5×10^5 atmospheres. Such temperatures and pressures are of the order of those to be expected in the interior of a planet about the size of Mars. Using thermodynamics, it can also be demonstrated that the conditions under which the elements distributed themselves between meteoritic phases varied from meteorite to meteorite in such a way that the greater the metal-phase content of the meteorite, the higher the temperature or the pressure must have been in order to establish the observed equilibrium.

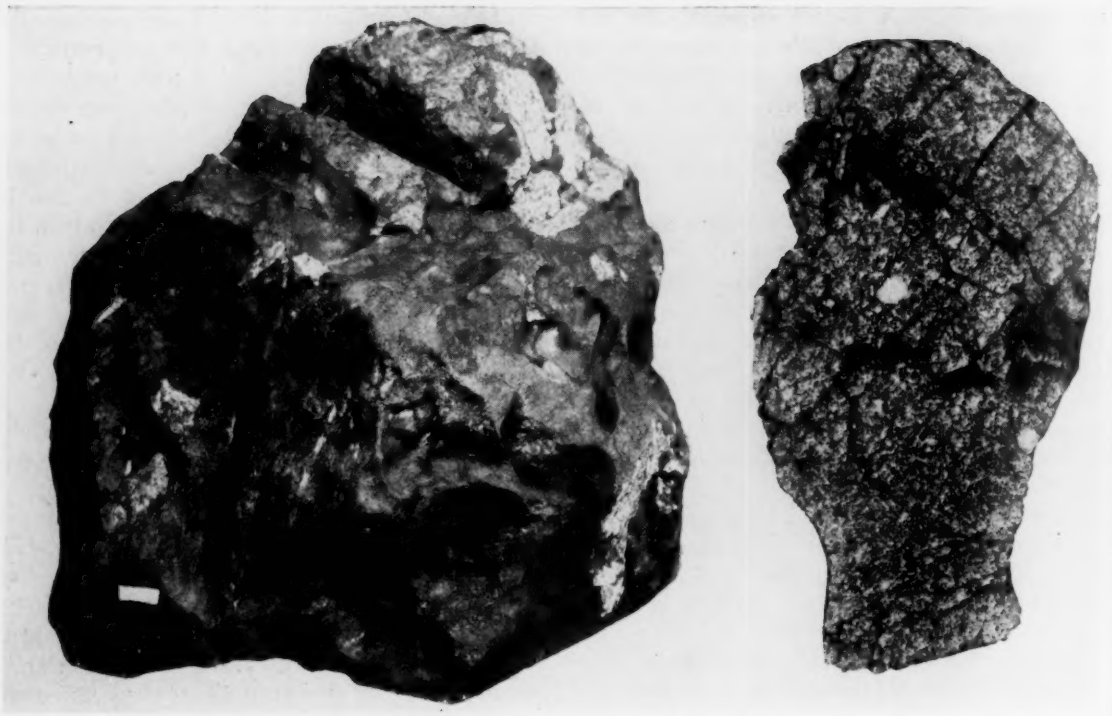
The conclusion to be drawn from thermodynamic evidence, then, is that meteorites were part of a planet, roughly the size of Mars. The planet was disrupted in some unknown manner, and since that time fragments of the planet have been striking the earth. In general, the greater the metal-phase content of a meteorite, the further from within the depths of the disrupted planet did it arise. The planet possessed a core of nickel-iron and a mantle of silicate phase containing dispersed metal.

On the basis of such findings, one can obtain some idea of the structure of the unknown planet by the simple process of averaging the compositions of meteorites over intervals of metal-phase content, keeping in mind that the greater the metal content, the closer one comes to the core. Table 2 shows the average metal-phase content and, for comparison, the compositions of igneous rocks and

TABLE 1

THE AVERAGE COMPOSITION OF STONY AND IRON METEORITES
(Percentage by Weight)

Element	Silicate Phase of Stones	Metal Phase of Stones	Iron Meteorites	Total Stony Meteorites	Igneous Rocks
Oxygen	43.12 ± 1.00	.....	.....	36.15 ± 0.89	46.49
Silicon	21.61 ± 0.19	.....	.....	18.12 ± 0.22	27.62
Combined iron	13.23 ± 0.42	.....	.....	14.21 ± 0.39	5.10
Combined nickel	0.39 ± 0.07	.....	.....	0.33 ± 0.06	0.01
Comb. cobalt	0.02 ± 0.01	.....	.....	0.02 ± 0.01	.....
Magnesium	16.62 ± 0.32	.....	.....	13.93 ± 0.29	2.11
Sulfur	.....	.....	.....	1.79 ± 0.08	0.05
Calcium	2.07 ± 0.21	.....	.....	1.74 ± 0.18	3.63
Aluminum	1.83 ± 0.15	.....	.....	1.53 ± 0.13	8.12
Sodium	0.82 ± 0.06	.....	.....	0.69 ± 0.05	2.85
Chromium	0.36 ± 0.06	.....	.....	0.30 ± 0.05	0.04
Manganese	0.31 ± 0.07	.....	.....	0.26 ± 0.06	0.09
Potassium	0.21 ± 0.02	.....	.....	0.18 ± 0.02	2.60
Phosphorus	0.17 ± 0.01	.....	.....	0.14 ± 0.01	0.13
Titanium	0.10 ± 0.03	.....	.....	0.08 ± 0.03	0.63
Hydrogen	0.07 ± 0.02	.....	.....	0.06 ± 0.02	.....
Metallic iron	.....	88.58 ± 0.55	90.78 ± 0.26	9.97 ± 0.69	.....
Metallic nickel	.....	10.68 ± 0.51	8.59 ± 0.24	1.20 ± 0.10	.....
Metallic cobalt	.....	0.70 ± 0.06	0.63 ± 0.02	0.08 ± 0.01	.....



A fall from Walters, Oklahoma. This stony chondrite meteorite weighs about 62 pounds. In the cross section, dark veins represent material from the outside which has penetrated the meteorite. White spots are chondrules, peculiar rounded granules.

plateau basalt (corresponding to very deep-seated rocks). It can be seen that the transition is a fairly smooth one, with silicon and oxygen diminishing steadily with increasing metal content, combined iron passing through a maximum, then decreasing steadily, magnesium increasing, then leveling off, and calcium passing through a maximum and then leveling off. These effects can be interpreted on the basis of pressure effects within the planet.

TABLE 2

AVERAGE COMPOSITION OF STONY METEORITES AS A FUNCTION OF METAL-PHASE CONTENT
(Percentage by Weight)

	Metal-Phase Content	Combined Iron	Oxygen	Silicon	Magnesium	Calcium
Igneous Rock	....	5.10	46.49	27.62	2.11	3.63
Plateau Basalt	....	8.09	45.41	23.64	2.95	5.78
	0.19	14.04	41.53	21.92	10.93	6.08
	1.06	14.79	41.39	21.87	12.22	3.29
	1.87	17.63	38.70	19.10	16.04	1.26
	2.78	20.67	37.78	18.00	11.86	2.16
	6.33	16.43	38.03	18.37	14.86	1.16
	9.97	14.16	36.44	18.36	14.38	1.23
	15.49	11.77	33.47	16.85	13.74	1.39
	22.48	8.90	30.71	15.90	13.71	1.37

The conclusion that must be drawn from thermodynamic and chemical studies on meteorites is that a parent planet once existed which was quite hot, probably similar to the earth in structure and in physicochemical characteristics. With its composition fixed by the fundamental abundances of the elements involved and the chemical processes connected with the initial formation of the planet, pressure effects produced segregations of matter within the body. The ratio of combined iron to metallic iron for the planet as a whole was fixed by the reducing conditions in existence during the beginning stages of the formation of the planet. Due to differences in ionic radii and density, however, localized shifts in equilibrium took place in such a way that the greater the distance from the core, the greater the state of chemical oxidation. As iron was the only major constituent of the magma that could readily give up oxygen, it played a major part in determining the states of the minor constituents of the planet.

Meteorites and the inner planets. Chemical evidence points strongly to a planetary origin of meteorites. Astronomical evidence is less clear-cut.

but recent observations by Bauer[4] add weight to the possibility from the astronomical point of view. The gap between Mars and Jupiter has been widely discussed as the possible location of the parent planet.

Clearly, the gross composition of meteoritic matter resembles what one might expect for the earth on the basis of the earth's known physical and chemical characteristics: gross density, ellipticity, surface composition, and seismic discontinuities. Could it be that the parent planet and the earth more than resembled each other, and actually possessed nearly identical gross compositions? A study of the densities of the three planets Venus, Earth, and Mars and the densities of meteoritic matter makes this possibility appear highly likely.

In Table 3 are given the relative masses and densities of the three planets in order of increasing mass (Mercury is omitted as its density is not known with sufficient accuracy). It will be noted that the density increases markedly with the mass. This phenomenon has commonly been attributed to major differences in compositions.

Let us assume, on the other hand, that all three planets, together with the meteoritic parent planet, possessed nearly identical compositions and structures; namely, a metallic iron-nickel core, of the composition of iron meteorites, and a silicate mantle containing dispersed metal phase, of the composition of stony meteorites. Under such circumstances, but for compression effects, all four planets would have identical densities. Compression effects, however, can play a major role, as evidenced by calculations by Jensen[5] on the density of iron at extremely high pressures. Utilizing Jensen's values, together with the known location of the boundary of the earth's core, and Bullen's[6] estimate of pressure versus distance within the core, one can calculate the density of the earth "uncompressed" and the ratio of metal phase to silicate phase in the earth as a whole. Using this ratio, one can then calculate the normal densities of Mars and Venus. On this basis, all three planets have an "uncompressed" density that is the same within the limits of the validity of such a calculation. In other words, *there is nothing inconsistent in the picture of all of the inner planets possessing nearly identical compositions, differing from one another in density almost entirely due to compression effects.* If this is true, then intensive study of the distribution of elements in meteorites should enable one to map out with fair confidence the internal structures of the inner planets.

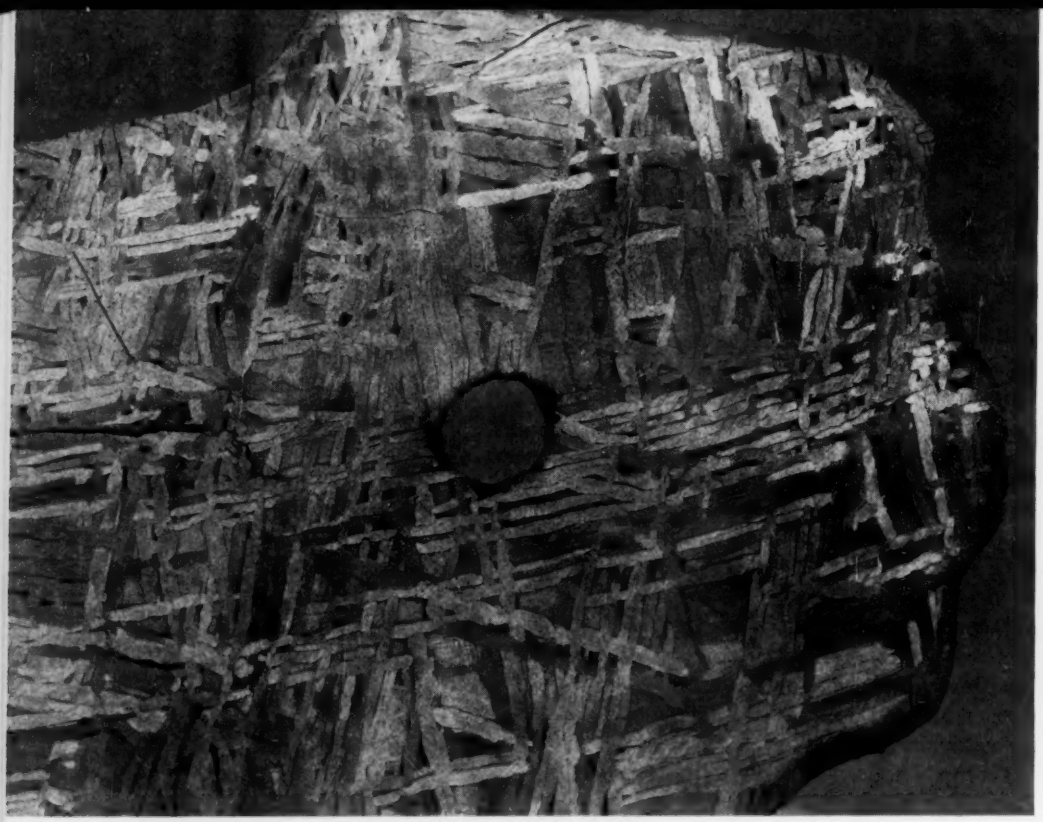
TABLE 3

PLANETARY MASSES AND DENSITIES

Planet	Mass	Density
Mars	0.11	3.96
Venus	0.81	4.86
Earth	1.00	5.52

Meteorite from Tennasilus, Esthonia. Rims around light areas (chondrules) are meteoritic iron, which rarely invades a chondrule.

Elements in meteorites and in the sun. It is possible to measure the relative abundances of certain elements in the sun by spectroscopic means. It is of interest to compare the composition of gross meteoritic matter with the composition of the sun, assuming a metal phase to silicate phase ratio for meteorites the same as that calculated for the earth. Comparison shows that, whereas the sun is primarily made up of light substances which condense only with difficulty, meteorites are composed primarily of heavier materials that are easily condensable, or that readily form easily condensable compounds (such as SiO_2). If one compares the abundances of the elements common to both meteorites and the sun, an interesting feature develops. Table 4 shows the abundance of certain elements in meteorites as measured chemically and computed in the method given above, together with the



An etched section of the Drum Mountains (Utah) meteorite, which at the date of its discovery (1944) was the eighth largest ever found in United States. This structural pattern is classified as a medium octahedrite, also known as Widmanstatten figures. The circular area in the picture is a cross section of one of the deep depressions that occurred on the surface of this meteorite.

abundances of the same elements in the sun as determined recently by Unsöld.[7] When one considers that the accuracy of the solar data is such that errors of the order of a factor of two might be involved in some cases, the agreement is quite good.

On the basis of this evidence, it is most tempting to postulate that the inner planets (including the meteorite parent planet) and the sun had a common origin from the same basic material, the inner planets retaining only the heavier, more easily condensable elements and compounds, and the sun retaining the lighter materials in addition.

In order to explain the much smaller densities of the outer planets (Jupiter, Saturn, Uranus, and Neptune), it is necessary to assume that owing to their size and the conditions of formation they retained substantial fractions of the lighter and more volatile substances during the process of planet formation. Space, however, does not permit a detailed discussion of the composition of the major planets in this article.

TABLE 4

COMPARISON OF ELEMENTS IN METEORITES AND IN THE SUN

ELEMENT	RELATIVE NUMBERS OF ATOMS	
	Meteorites	Sun
Na	462	750
Mg	8,870	12,000
Al	882	850
Si	10,000	7,800
K	69.3	64
Ca	670*	670*
Sc	0.18	0.87
Ti	26	36
V	3	4.4
Cr	93	150
Mn	81	120
Fe	26,200	21,000

* The solar values have been adjusted so that meteoritic and solar calcium are equal. Silicon in meteorites = 10,000.

The relative abundances of elements. Measurement of the abundances of elements in the sun and in the stars by spectroscopic means is quite difficult, and the number of elements that can be measured even approximately is limited. Spectroscopic evidence indicates, however, that the composition of the visible universe might well be fairly uniform throughout, with the exception of lighter elements whose abundances are changing because of thermonuclear transformations. If this is true, meteorites can be used to good advantage in measuring the relative abundances of the less volatile elements. Fortunately, the vast majority of the elements fall into this category. The abundance values as determined using meteoritic data can then be coupled with the spectroscopic solar and stellar values for the cases where meteorites cannot be used with validity. On this basis, by suitably combining meteoritic and spectroscopic data, a table of "Cosmic Relative Abundances"

TABLE 5

COSMIC RELATIVE ABUNDANCES
(Atoms per 10,000 Atoms of Si)

ELEMENT	ABUNDANCE
H	1.2×10^8
He	2.8×10^7
C	75,000
N	160,000
O	210,000
F	10
Ne	$10^3 - 10^5$
Na	462
Mg	8,870
Al	882
Si	10,000
P	170
S	3,300
Cl	250
A	190
K	69.3
Ca	670
Sc	0.18
Ti	26
V	3
Cr	93
Mn	81
Fe	26,200
Co	157
Ni	2,130
Cu	6.9
Zn	2.6
Ga	0.54
Ge	0.39
As	0.73

can be compiled. Table 5 shows the first section of such a compilation.

In view of the fact that the vast majority of elements are present in meteorites to an extent of but a few parts per million, the analytical problems involved are large. But fundamentally there are no limitations to such an approach, and the time should not be too far distant when a precise table of elemental abundances is available to aid in the solution of many astrophysical problems. The surface of the field of meteoritics as applied to astrophysical problems has barely been scratched. It is clearly a field that will yield rewarding information in the future.

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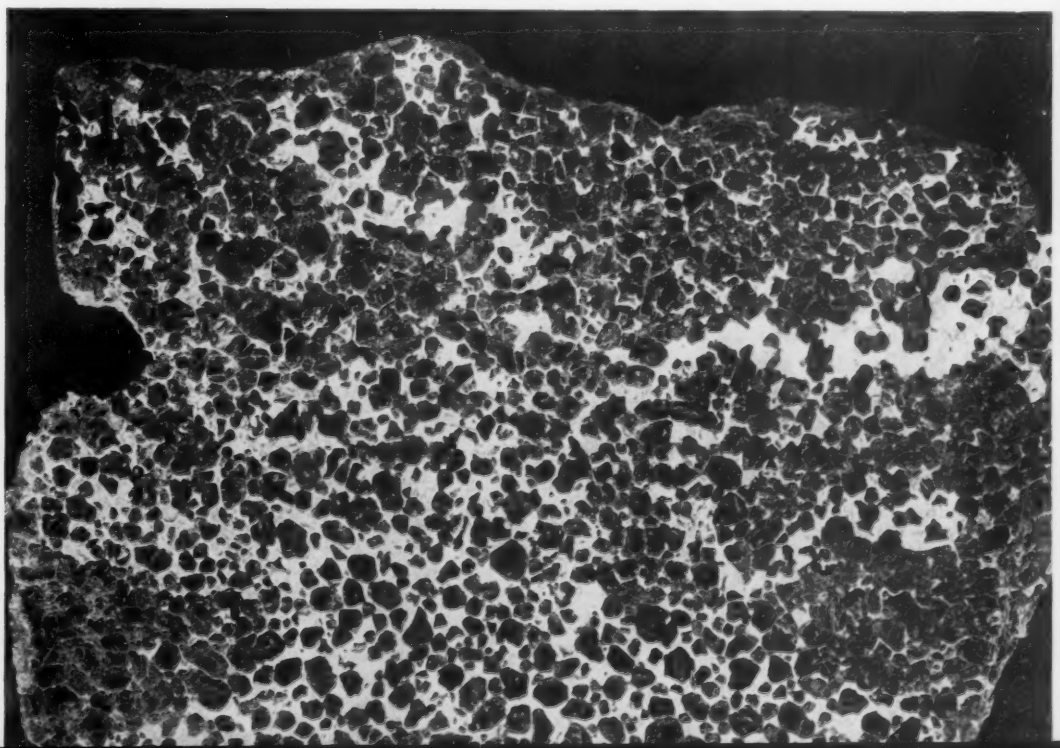
Mount Vernon meteorite. Knowledge of the existence of this olivine and iron, or "pallasite," meteorite (*below*) dates from 1868, but it was not recognized as a meteorite until 1902. Weighing originally about 351 pounds, it was found in one mass near Hopkinsville, Kentucky. Cross section shows (*white areas*) iron and nickel alloy.



PROBABILITY, RARITY, INTEREST, AND SURPRISE

WARREN WEAVER

Dr. Weaver (Ph.D., University of Wisconsin, 1921) taught at his alma mater from 1920 to 1932, when he resigned as chairman of the Department of Mathematics to become director of the Division of Natural Sciences of the Rockefeller Foundation. During World War II he served on the National Defense Research Committee, the Naval Research Advisory Committee, and as a member of the War Department Research Advisory Panel.

THE unusual is often interesting. Moreover, the frequency definition of probability, which is the one consciously or tacitly used in all applications to experience, makes it clear that an improbable event means precisely a rare event. Hence, an improbable event is often interesting. But is an improbable event always interesting? We shall see that it is not. If an event actually occurs, and if its probability, as reckoned before its occurrence, is very small, is the fact of its occurrence surprising? The answer is that it may be, or it may not be.

Suppose one shuffles a pack of cards and deals off a single bridge hand of thirteen cards. The probability, as reckoned before the event, that this hand contain any thirteen specified cards is 1 divided by 635,013,559,600. Thus the probability of any one specified set of thirteen cards is, anyone would agree, very small.

When one hand of thirteen cards is dealt in this way there are, of course, precisely 635,013,559,600 different* hands that can appear. All these billions of hands are, furthermore, equally likely to occur; and one of them is absolutely certain to occur every time a hand is so dealt. Thus, although any one particular hand is an improbable event, and so a rare event, no one particular hand has any right to be called a surprising event. Any hand that occurs is simply one out of a number of exactly equally likely events, some one of which was bound to happen. There is no basis for being surprised at the one that did happen, for it was precisely as likely (or as unlikely, if you will) to have happened as any other particular one.

So we see that an event should not necessarily be viewed as surprising, even though its a priori probability is very small indeed. If you ever happen to be present at the bridge table when a hand of thirteen spades is dealt, remember that what you ought to say is this: "My friends, this is an improbable and a rare event: but it is *not* a surprising event. It is, however, an interesting event."

For it remains stubbornly true that, under the circumstances described, we are sometimes interested in the outcome, and sometimes uninterested. It seems clear that this occurs because the hands, although equally likely, are just not equally interesting. Thirteen spades *is* an interesting hand, and indeed a Yarborough with no cards above the ten, is interestingly poor; whereas millions of the hands are dull and wholly uninteresting messes.

On the other hand, think of a round disk of metal, like a coin except with perfectly plane faces and no milling on the edge. If this coin-disk were tossed, and landed in an almost perfectly vertical position, with practically no spin, it would remain standing on its edge. One could easily adjust the thickness and the mode of spinning so that the probability of its coming down and standing on its edge would be, say, one in a billion.

When this metal disk is tossed, there are, then, three possible outcomes. It may come up heads (for which the probability differs from one half by one half of a billionth), or it may come down tails (which is exactly as likely as heads), or it may stand on its edge (for which the probability is one one-billionth).

Either heads or tails would be probable, not rare, uninteresting, and not surprising. Standing on edge would be improbable, rare, interesting, and surprising. But this surely surprising event is more than 635 times as probable as would be an uninteresting hand in our previous illustration. So standing on edge is not surprising just because it has a low probability. Why is it surprising?

It is surprising, of course, not because its probability is small in an absolute sense, but rather because its probability is so small as compared with the probabilities of any of the other possible alternatives. Standing on edge is half a billion times as unlikely as the only other two things that can happen, namely, heads or tails: whereas the dull hand is precisely as likely (or unlikely) as any of the possible alternative outcomes of the dealing process.

* Order of appearance of cards in the dealing process is not taken into account: only the final constitution of the hand. Two hands are, of course, "different" if they differ in one or more cards.

Thus one concludes that *probability* and *degree of rarity* are essentially identical concepts: that a *rare* event is *interesting* or not depending on whether you consider it interesting or not† and that an event is *surprising* only providing its probability is very small as compared with the probabilities of the other accessible alternatives. This requires that a *surprising* event be a *rare* event, but it does not at all require that a *rare* event be a *surprising* event.

It is easy to make this idea more precise, and to define a Surprise Index (S. I.) for an event. To do this, it is necessary to say a preliminary word about what is called, in probability theory, mathematical expectation, or simply expectation.

If a probability experiment can result in any one of n ways, W_n, and if the a priori probabilities of these various ways are $p_1, p_2, \ldots p_n$, then any quantity Q which takes on the values $Q_1, Q_2, \ldots Q_n$ as the experiment eventuates in its possible ways W_n is said, relative to this experiment, to have the expected value

$$E(Q) = p_1Q_1 + p_2Q_2 + \ldots + p_nQ_n.$$

This definition sounds a little formidable, but the essence of the matter is really very simple. For, since the probabilities can be thought of as the relative frequencies of occurrence of the various outcomes, the formula gives a weighted average of Q, weighting each possible value of Q proportionately to the frequency with which it may be expected to occur. That is to say, the mathematical expectation of any quantity Q is simply the average value that Q may be expected to tend toward as the number of experiments becomes large.

Now, when a probability experiment is carried out, one of the ways W necessarily occurs—that is, in fact, what we mean by "doing the experiment." This way W (the one that actually happens) had a certain a priori probability p that it would occur; and, since it has occurred, we can say that we have realized, in this experiment, a probability p. How much probability can one expect on the average to realize in a given experimental setup? We have just seen that mathematical expectation answers precisely this kind of question. We must calculate the expected value of the probability. Thus

$$E(p) = p_1^2 + p_2^2 + \ldots + p_n^2$$

is the average amount of probability we can expect to realize per trial of the experiment in question. And the way, say, W_i that the experiment actually comes out, is to be considered surprising, according to whether the Surprise Index

$$(\text{S.I.})_i = \frac{E(p)}{p_i} = \frac{p_1^2 + p_2^2 + \ldots + p_n^2}{p_i}$$

is large compared with unity or is not. The S. I., as so defined, obviously measures whether the probability realized, namely, p_i, is small as compared with the probability that one can expect on the average to realize, namely, $E(p)$. If this ratio is small and S. I. correspondingly large, then one has a right to be surprised.

For the card-hand illustration, all the p_i are equal, and at once it turns out that the Surprise Index for any such hand is equal to unity. Thus in this experiment no outcome, however rare it may be, however interesting or uninteresting, is in the least surprising.

For the coin illustration, however, if we say that W_1 denotes a head, W_2 a tail, and W_3 standing on edge, then it is at once calculated that $(\text{S.I.})_1$ and $(\text{S.I.})_2$, which are equal, differ from unity only in the eleventh decimal place; whereas $(\text{S.I.})_3$ is within one of being a half million. Standing on edge is, therefore, decidedly surprising, as well as being improbable and interesting; whereas neither a head nor a tail is at all surprising.

It was remarked, earlier, that a hand of thirteen spades is rare and interesting, but should not be viewed as surprising. How about two hands of solid trumps in one evening or, what is entirely equivalent, any two identical hands in one evening? That is a very different matter! If an experiment consists of dealing just two hands, then this experiment can come out in either one of two ways. It may, way W_1, result in two unlike hands, or it may, way W_2, result in two identical hands. Since W_2 has one chance in many billions of occurring, it is easily calculated that the Surprise Index for W_1 is essentially unity, whereas the S. I. for W_2 is an extremely large number. Thus, two unlike hands in succession are not in the least surprising, whereas two alike in succession are very surprising indeed.

This calculation also indicates that there is in fact a viewpoint which justifies surprise at a single hand of thirteen spades. Suppose, for example, that one is so much interested in a perfect hand that he lumps together all imperfect ones. That is, for this person there are only two sorts of hands, perfect and imperfect. For him the deal of a hand comes out in only two ways. The calculation of the Surprise Index corresponding to these two ways is, of course, exactly that given in the preceding paragraph. The S. I. for an imperfect hand

† It may perfectly well (alas for after-dinner conversation) be interesting to you, say, because it happened to you before, and not at all interesting to anyone else.

is essentially unity, whereas the S. I. for a perfect hand is more than 635,000,000,000. It is therefore necessary to modify the general statements made previously and say that when an improbable event is so interesting that all its alternatives are lumped together as events so dull as to be indistinguishable, then the interesting event may thereby become a surprising event.

For those who play with statistics it may be worth while to point out that it seems to be difficult indeed to calculate, in general terms, the expectation $E(p)$ of the probability for either the binomial or the Poisson case. The resulting series involves squares of factorials, which are most unpleasant.‡

For the normal case, it is trivially easy to show that the expectation in the probability density, as expressed in terms of the standard deviation σ, is given by $1/2\sigma\sqrt{\pi}$. The Surprise Index for a deviation x from the mean is found at once to be equal to $\sqrt{2}e^{-x^2/2\sigma^2}$. Thus the Surprise Index is large compared with unity provided x is large compared with σ; which is of course just what one would expect.

This last calculation, because of long experience with, and feel for, standard deviation, gives one a little notion as to what would be a sensible agreement, throughout this discussion, as to what "large" ought to mean. A Surprise Index of 3 or 5 is surely not large: one of 10 begins to be surprising: one of 1,000 is definitely surprising: one of 1,000,000 or larger is very surprising indeed: one of 10^{12} would presumably qualify as a miracle.

Also, for those who have a more professional interest in statistics, it may be interesting to mention that the ideas here presented have some rather remote connection with the concept of "likelihood" as introduced more than twenty years ago by R. A. Fisher. The simplest case to which Fisher's notions apply is similar to the cases here considered in that a probability event has occurred and one wishes to draw certain inferences from this fact of occurrence. But, in the Fisher case, the probability of occurrence depends upon some unknown parameter, and the main point is that one is attempting to infer some information concerning this unknown parameter. In the cases to which the ideas of the present article apply, there is no such unknown parameter, it being assumed that all the necessary facts are at hand to permit a precise calculation of the probabilities in question. In actual experiments, it is seldom if ever true that all the facts for an absolutely precise calculation of the probabilities are indeed available. Thus, in a card experiment, one could not be entirely certain that the shuffling process is perfect. Indeed, one could have quite an extensive argument as to what "perfect" means. Thus, in actual experiments, it is quite likely to be true, at least in a precise sense, that certain unknown parameters are present: and therefore the Fisher theory is, if one wishes to be quite precise, necessary. I do not think that this destroys the interest in the above remarks, which do apply to those many instances in which one is quite willing to simplify the situation by agreeing that the facts concerning the probabilities are known.

The excuse for making these rather obvious, but still not trite, remarks to a general audience is that all scientists have to be concerned with probability; and it is not altogether rare, although surely surprising, to find scientists who are surprised to find improbable things occurring. They always have a right to be interested, but only seldom do they have a right to be surprised.

It is hoped, then, that the considerations here presented will give some pause to those who say too glibly and too simply: "But the theory of this experiment *must* be accepted, for the probability of the results occurring by pure chance is only 10^{-20}. Surely nothing as improbable as that could ever occur." The mere fact that a probability is low should not of itself lead to incredulity. In fact, life consists of a sequence of extremely improbable events. One must always, before concluding that a certain event is surprising, examine the probability of the alternatives. For there are circumstances, and an evening of hands of bridge is a very good example, under which the occurrence of an over-all event of fantastically small probability is, in fact, inevitable.

‡ I have spent a few hours trying to discover that someone else had summed these series and I spent substantially more trying to do it myself; I can only report failure, and a conviction that it is a dreadfully sticky mess.

ON LIVING IN THE BIOSPHERE

G. E. HUTCHINSON

In the article below, which is from a paper presented in the symposium on "The World's Natural Resources" (AAAS Centennial, September 13–17), Professor Hutchinson claims originality only for some of the computations and part of the interpretation—most of the fundamental data are easily accessible in the scientific literature. Professor Hutchinson is on the staff of the Department of Zoology at Yale and consultant in biogeochemistry, American Museum of Natural History.

IN discussing the subject of "The World's Natural Resources," I want first to make a number of general observations that will provide an intellectual framework into which our developing knowledge, both academic and practical, may be fitted. We live in a rather restricted zone of our planet, at the base of its gaseous envelope and on the surface of its solid phase, with temporary excursions upwards, downwards, or sideways onto or into the oceans. These regions in which we can live and which we can explore are characterized by their temperature, which does not depart far from that at which water is a liquid, and by their closeness to regions on which solar radiation is being delivered. This zone of life is spoken of as the biosphere. Within it, certain natural products can be utilized in both biological and cultural life. It is customary to consider these resources as either material or energetic, but the two categories are not easily separable; contemporary solar radiation is an energetic resource, coal and oil are to be regarded as material resources valuable for their high energy content, which we may call, epigrammatically, fossil solar radiation. There is a third very important though inseparable aspect, namely, the pattern of distribution. Most fossil sunlight, or chemical energy of carbonaceous matter, is diffused through sedimentary rocks in such a way as to be useless to us. Schrödinger says that we feed on negative entropy, and I am almost tempted to regard pattern as being as fundamental a gift of nature as sunlight or the chemical elements.

The first major function of the sunlight falling on the earth's surface is as the energy of circulation of the oceans and atmosphere. The second is to increase the mobility of water molecules, to become latent heat of evaporation, and so to keep the water cycle operating. The third major function is photosynthesis. Apart from atomic energy, and a little volcanic heat which presumably is actually of radioactive origin, all industrial energy is solar and due to one or the other of these three processes.

The material requirements of life are extremely varied. Between thirty and forty chemical elements appear to be normally involved. Industrially, some use appears to be found for nearly all the natural elements, and some of the new synthetic ones also. Looking at man from a strictly geochemical standpoint, his most striking character is that he demands so much—not merely thirty or forty elements for physiological activity, but nearly all the others for cultural activity. What we may call the anthropogeochemistry of cultural life is worth examining. We find man scurrying about the planet looking for places where certain substances are abundant; then removing them elsewhere, often producing local artificial concentrations far greater than are known in nature. Such concentrations, whether a cube of sodium in a bottle in the laboratory, or the George Washington Bridge, have usually been brought into being by chemical changes, most frequently reductions, of such a kind that the product is unstable under the conditions in which accumulation takes place. Most artifacts are made to be used, and during use the strains to which they are submitted distort them, and they become worn-out or broken. This results in a very great quantity of the materials that are laboriously collected being lost again in city dumps and automobile cemeteries. The final fate of an object may depend on many factors, but it is probable that in most cases a very large quantity of any noncombustible, useful material is fated to be carried, either in solution or as sediment, into the sea. Modern man, then, is a very effective agent of zoogenous erosion, but the erosion is highly specific, affecting most powerfully arable soils, forests, accessible mineral deposits, and other parts of the biosphere which provide the things that *Homo sapiens* as a mammal and as an educatable social organism needs or thinks he needs. The process is continuously increasing in intensity, as popu-

lations expand and as the most easily eroded loci have added their quotas to the air, the garbage can, the city dump, and the sea.

The most important general consideration to bear in mind in discussing the dynamics of the biosphere and its inhabitants is that some of the processes of significance are acyclical, and others, to a greater or less degree, cyclical. By an acyclical process will be meant one in which a permanent change in geochemical distribution is introduced into the system; usually a concentrated element tends to become dispersed. By a cyclical process will be meant one in which the changes involved introduce no permanent alteration in the large-scale geochemical pattern, concentration alternating with dispersion. The cyclical processes are not necessarily reversible in a thermodynamic sense; in fact, they are in general no more and no less reversible than the acyclical. Most of the cyclical processes operate because a continuous supply of solar energy is led into them, and sunlight will provide no problems for the conservationist for a very long time. It is important to realize that most of the acyclical processes are so slow that man appears as an active intruder into a passive pattern of distribution. They are safer to disturb because we know what the result of the disturbance will be. If we mine the copper in a given region sufficiently assiduously, we know that ultimately there will not be any more copper available there. Cyclical processes involve complex circular paths, regenerative circuits, feedback mechanisms, and the like. Small disturbances of such processes may merely result in small temporary changes, with a rapid return to the previous steady state. This has been beautifully demonstrated in the experiments of Einsele, who added single massive doses of phosphate to a lake, changing for a time, but only for a time, its entire chemistry and biology. This stability does not imply that if large disturbances strain the mechanism beyond certain critical limits very profound disruption will not follow; in fact, the very self-regulatory mechanisms that give the system stability against small disturbances are likely to accentuate the disruption when the critical limits are transcended. In disturbing cyclical processes we usually do not know what we are doing.

The most nearly perfect cyclical processes are those involving water and nitrogen. Some losses to the sediments of the deep oceanic basins must occur, but they are very small and are doubtless fully balanced or more than balanced by juvenile water and perhaps by molecular nitrogen and ammonia of volcanic origin.

The least cyclical processes are those in which material is removed from the continents and deposited in the permanent basins of the ocean. With one or two exceptions, the delivery to the deep-water sediments of the ocean is of little significance. Most of the mechanical and chemical sedimentation in the oceans takes place in relatively shallow water. The uplifting of shallow water sediments constitutes an important method of completing cycles. One, and perhaps two, exceedingly important elements are, however, sedimented less economically. Calcium, during the Paleozoic, was mainly precipitated in shallow water, but since the rise of the pelagic foraminifera in the Mesozoic, a great deal of calcium, along with an equivalent amount of carbon and oxygen, has been continually diverted to regions from which it is unlikely ever to be removed. At present the sedimentary rocks of the world are an adequate biological and commercial source of calcium, but, with progressive orogenic cycles, less and less of the element will be uplifted (Kuenen), and whatever organisms inherit the earth in that remote future will have to face the problem of the biosphere "going sour on them." For phosphorus, the case is less well established, but there is probably a slow loss in the form of sharks' teeth and the ear bones of whales (Conway), which are very resistant and which are known to be littered about on the floor of the abysses of the ocean.

It is desirable to consider two of the main geochemical cycles in order to gain an idea of the effect of man upon them. It must be admitted that we are ignorant of many matters of importance here. In the cycle of carbon we have a remarkable, possibly a unique, case in which man, the miner, increases the cyclicity of the geochemical process. It is generally admitted that our available store of carbon is ultimately of volcanic origin. A steady stream of carbon dioxide and lesser amounts of methane and carbon monoxide are entering the atmosphere from volcanic vents. Part, probably a major part, of this carbon dioxide is ultimately lost to the marine sediments as limestones; a very small part of it is then returned to the air, wherever lime kilns are in operation. Another part of the CO_2 entering the atmosphere is reduced in photosynthesis. A part of the organic matter so formed is fossilized, and a small part of this fossilized organic carbon is available as fuel, in the form of coal and oil. At the present time it appears that the combustion of coal and oil actually returns carbon to the atmosphere as CO_2 at a rate at least a hundred times greater than the rate of loss of all forms of carbon, oxidized and reduced,

to the sediments (Goldschmidt). This particular process obviously cannot go on indefinitely. It concerns only the reduced carbon; to complete the cycle in the case of oxidized carbon, a great deal of energy would have to be supplied. It concerns only the reduced carbon which is aggregated. The poorest sources would be the poorest exploitable oil shales. Most of the reduced carbon is much more dispersed than this; in making an estimate of the total reduced carbon of the sediments, the commercially usable fuels constitute a negligible fraction that need not be considered.

Although the rate at which carbon dioxide is returned to the air by the human utilization of fossil fuels is so very much greater than is the primary production of carbon dioxide from volcanic sources, the rate is evidently a very small fraction—of the order of 1 percent—of the rate of photosynthetic fixation and subsequent respiratory liberation of CO_2 by the organisms of the earth. Since about 1890 a slight increase in CO_2 content in the air, at least at low altitudes over the land surfaces of the Northern Hemisphere, has been noted. This has been attributed to the accumulation of industrially produced CO_2, as the quantity of CO_2 that has appeared in the atmosphere is of the same order of magnitude as the total combustion of fuel (Callendar). In view of the small fraction of the total CO_2 production that industrial output represents, it seems very unlikely that merely adding an extra percent to the natural biological production should overload the cyclic process, so that it rejects quantitatively the additional load. It is known that the air at high altitudes still shows nineteenth-century values. The most reasonable explanation of the observed increase is that the photosynthetic machinery of the biosphere has been slightly impaired, probably by deforestation. It is clear that, in any intelligent long-term planning of the utilization of the biosphere, an extended study of atmospheric gases is desirable, even though for the moment it seems unlikely that the observed change is a particularly serious symptom.

The only other cycle that can be considered in any great detail is that of phosphorus. The chief event in the geochemical cycle of phosphorus is the leaching of the element from the rocks of the continents, and its transport by rivers to the sea. At the present time the rate of this transportaion is of the order of 20,000,000 tons of phosphorus per year for the entire earth. Part of this phosphorus, when it enters the sea, will ultimately be deposited in the sediments of the depths of the ocean. Such phosphorus will probably be largely lost to the geochemical cycle, as has just been indicated. The sedimentary rocks of the continents, therefore, will gradually lose phosphorus; there is some evidence that this has actually occurred (Conway). The main return path is by the uplifting of sediments formed in continental seas, which then undergo renewed chemical erosion. Of particular interest are methods by which concentrated phosphorus can be returned to the land surfaces. As far as is known, there are two such methods: The first is the formation of phosphatic nodules and other forms of phosphate rock in regions of upwelling in which water at a low pH, rich in phosphate, is brought up to the surface of the sea. The pH falls and an apatite-like phosphate is deposited. When the sea floor is later elevated, a commercial deposit may result. The second method is by the activity of sea birds, such as the guano birds of the Peruvian coast. There is little or no unequivocal evidence that guano deposits of great extent were formed prior to the late Pliocene or Pleistocene. Some of the well-known occurrences of rock phosphate, such as that of Quercy, have been explained in this way, but they are certainly not typical guano deposits. During the late Tertiary and Pleistocene, an extraordinary amount of phosphate was deposited on raised coral islands throughout the world, and bird colonies seem to provide the only reasonable agencies of deposition. The great deposits of Nauru, Ocean Island, Makatea, Angaur, the Daito Islands, Christmas Island south of Java, Curaçao, and some of the other West Indies all seem to have been formed in this way. This process is as characteristic of the time as is glaciation, though less grandiose. Its meaning is not clear, but it is probably connected with changes in vertical circulation of the ocean as glaciopluvial periods gave place to interpluvials. Today in certain regions massive amounts of guano are deposited, and it is probable that the oceanic birds of the world as a whole bring out from several tens to several hundreds of thousands of tons of phosphorus and deposit it on land. Only about 10,000 tons of the element are delivered in places where it is not washed away and where it can be carried by man to fertilize his fields.

The main processes that tend to reverse the phosphorus depletion of the continents are, therefore, the deposition of marine phosphorites on the continental shelves and subsequent elevation, and the formation of guano deposits. Both processes are evidently intermittent, and are quantitatively inadequate to arrest deflection of the element into the permanent ocean basins. Man contributes both

to the loss and to the gain of phosphorus by land surfaces. He quarries phosphorite, makes superphosphate of it, and spreads it on his fields. Most of the phosphorus so laboriously acquired ultimately reaches the sea. At present the world's production of phosphate rock is about 10,000,000 tons per annum. This contains from 1,000,000 to 2,000,000 tons of elementary phosphorus. Human activity probably, therefore, accounts for from 5 to 10 percent of the loss of phosphorus from the land to the sea. Man also contributes to the processes bringing phosphorus from the sea to the land. This is done by fisheries. The total catch for the marine fisheries of the earth is of the order of $25–30.10^6$ tons of fish, which corresponds to about 60,000 tons of elementary phosphorus. The human, no less than the nonhuman, processes tending to complete the cycle seem miserably inadequate. It is quite certain that ultimately man, if he is to avoid famine, will have to go about completing the phosphorus cycle on a large scale. It will be a harder task than that of solving the nitrogen problem, which would have loomed large in any symposium on "The World's Natural Resources" fifty years ago, but possibly an easier problem than some of the others that must be solved if we are to survive and really become the glory of the earth.

The population of the world is increasing, its available resources are dwindling. Apart from the ordinary biological processes involved in producing population saturation already known to Malthus, the current disharmony is accentuated by the effect of medical science, which has decreased death rates without altering birth rates, and by modern wars, which one may suspect put greater drains on resources than on populations. Terrible as these conclusions may appear, they have to be faced. The results of the interaction between population pressure and decline of resource potential are further partly expressed in such wars, which are pathological expressions of attempts to cope with these and other problems and which now invariably aggravate the situation. It is evident that the fundamental causes of war lie in those psychological properties of populations which make them attempt solutions in a warlike manner, and *not* in the existence of the problems themselves. The two problems of war and of resources are, however, at present very closely interrelated; it is probably impossible to find a solution for one without progress in the solution of the other. Any kind of reasonable use of the world's resources involves better international relations than now exist. It is otherwise impossible to operate on a planetary basis, or to avoid the fearful material and spiritual expense of living in a world divided into two armed camps. The lack of international trust is the first difficulty in achieving rational utilization of the resources of the world; the second difficulty for the United States is what may be called the problem of the transition from the pioneering to the old, settled community. For a pioneer, life may be hard, but in good country there is "plenty more where these came from," whether lumber or buffalo tongues or copper. This attitude is incorporated into popular thought very deeply; in a crude form, it is now completely destructive, but it seems possible that attitudes might be developed that could utilize such a point of view. There is at least one thing of which we have plenty more than we use, whether we call it Yankee ingenuity, American know-how, or the human intellect. In some industrial fields there has been a notable series of triumphs of the kind that really will give plenty more of a number of things, and give them in a cyclic manner. The rise of the magnesium industry, and the utilization of the magnesium of sea water as a source of the metal is an interesting example. The production of plastics, though it is probably at the moment not as geochemically economical as it should be, is another. These, along with the development of silicate building materials not involving any particularly uncommon elements, all point the way to the kind of material culture that permits a reasonably high living standard, at least in certain directions, without devastation of the earth.

The future outlook for the world, particularly in food resources, has been put before the public in several recent books, notably those of Fairfield Osborn and William Vogt. Anyone with any technical knowledge understands that the dangers described in these books are real enough, more real and more dangerous perhaps than the threat of an atomic world war. The problem of getting action to forestall such dangers in a culture that has developed under conditions of potentially unlimited abundance just around the corner, is obviously extremely difficult. I do not think it is impossible. The first requirement is a faith that the job can be done. The very difficulty of the task, its apparent impossibility, may here and now prove a challenge that brings the desired response. The difficult we do at once, the impossible takes a little longer. I doubt that a direct appeal to fear will produce any results except a disbelief in the prophets of doom. Cassandra seems even more unpopular in modern America than in ancient Ilium. There would seem to be forces operating in society which tend to reverse the destructive

processes, or which could be made to do so. One of the most immediate needs is to find out what they are and do everything possible to strengthen them. I will give one example. A number of industrial concerns—particularly in the chemical and pharmaceutical field, but also some engineering and publishing firms—have used in their advertising a legitimate pride in the learning, skillful research, and development that have gone into the manufacture of their product. It is quite certain that there are many cases in which one particular product, the result of considerable research, and of taking risks in development, actually reduces the drain on the natural resources of the country and the world. I should like to see a small systematic experiment, on the part of some such concern, in advertising in which it is pointed out that by buying this product one is letting the industrial skill behind the product operate for the benefit of one's children. I am fully aware that if this point of view were worked up skillfully enough by a few responsible, public-spirited corporations and put into a form that would pay the corporation, as well as the country and the world, there would be other less responsible concerns who would use the method when their product is actually not one sparing natural resources. This, however, seems to be a lesser evil than the total neglect of the commercial advertising field, which is one of the most potent in determining the values of the public and which at present is largely disruptive. I do not doubt that a professional cultural anthropologist could pick out a great many other fields that could be used to promote the idea of an expanding economy based on an abundance of human ingenuity rather than on an excess of raw materials.

There is one further point that I should like to develop. Though the pursuit of happiness is embedded deeply in the constitutional foundations of the United States, we do not know much about it. It is fairly certain that no metric exists that can be applied directly to happiness, but intuitively we may proceed a little way by arguing as if such a metric could exist. It is obvious that only a very few people, with a genius for sanctity, can be happy if half-frozen and starving. If the temperature be raised, and the food supply and other amenities increased, the possibility of happiness is obviously at first also increased, but beyond a certain increase in the environmental resources available no further increase in happiness would be expected and we might begin to look for a decline. The image of an overheated kitchen, filled with too much electrical equipment and catering to overfed people, will, if adequately evoked, have a nightmarish quality. In more formal language, if we could find a function of the environmental resources that expressed the relationship of happiness to those resources, it is reasonably certain that the function would not be monotonic. For every resource there seems likely to be an optimum level of consumption, but we do not know if this optimum level is, in any particular case, widely exceeded, so that gross overutilization is actually producing avoidable distress.

The problem is not by any means as simple as that of determining discrete optima. In any given society all the cultural values are probably interrelated to form a coherent system, so that the existence of one set of values may greatly modify the others. If, as seems possible, our attitude toward food leads a considerable section of our population to be definitely overweight, it is legitimate to inquire to what extent, by changes in the upbringing of our children, the psychological needs filled by food can be satisfied in other ways so that the psychologically optimum intake falls to a level nearer the psychological optimum for the individual and the moral optimum for the world. We might ask, for instance, how we can substitute the delights of ballet and Mozartian opera, which are geochemically very cheap, for part of those provided by hot dogs or apple pie and ice cream, which may in the long run prove too expensive to use except as a source of energy and essential nutrients. This example is chosen with a view to indicating that the kind of substitutions that might be considered need not be in the least puritanical. It may appear overintellectualized, but that at least is a guarantee that it is not inhuman. Indeed it raises the very interesting problem that those of us in the educational world have to face, namely, why we are raising a generation in the belief that the majority of constructive, complicated, difficult activities are boring duties, when the same generation shows us that in certain specific fields, mainly concerned with electronic amplifiers and with the explosive combustion of hydrocarbons, complicated activity can be very entertaining. What we have to do is to show by example that a very large number of diversified, complicated, and often extremely difficult constructive activities are capable of giving enormous pleasure. This is, in fact, the reason why it is essential that the teachers in our colleges and universities should be enthusiastic investigators in the fields of scholarship or practitioners and critics in their arts. It ought to be possible to show that it is as much fun to repair the biosphere and the human societies within it as it is to mend the radio or the family car.

WOOD IN AN INDUSTRIAL WORLD

J. A. HALL

Dr. Hall (Ph.D., Wisconsin, 1934) is director of the Pacific Northwest Forest and Range Experiment Station, USDA, at Portland, Oregon. Speaking as a biochemist, he delivered the address from which this article is taken during the Centennial Celebration of the AAAS in Washington, September 13–17.

WE LIVE in the industrial revolution that began early in the nineteenth century and was cradled in Western Europe. Within the short period of a century and a half we have witnessed a violent transformation in the economic and social aspects of human society influenced by a completely revolutionized technology. We have also seen an explosive upsurge in world population that has influenced the course of the industrial revolution itself and made necessary the extension of the industrial system to the outermost regions of human habitation. Since what we know as the industrial system seems to have become a permanent part of organized human life, and since its existence and perpetuation depend so much upon the abundant availablity of raw materials, it is peculiarly appropriate that we should examine the place in this system of one of our most abundant raw materials—wood.

WOOD AND THE INDUSTRIAL REVOLUTION

Historically, wood was important in the early beginnings of the industrial revolution. It had been a vital fuel stuff, an important part of the crude machinery that antedated the advent of modern, more complex machines, the material of which ocean-going transport was built, and the principal component of all land-borne, wheeled transport. The basic raw materials for the development of the modern factory system were present in Europe and North America, but they awaited the catalytic effect of the application of steam to power machinery before they could be put together into the industrial system that arose rapidly on both continents. The requirements for industrialization are and have always been (1) a base of scientific knowledge, especially in the fields of application of energy in the transformation of materials; (2) large sources of available energy; and (3) raw materials of varied nature and large supply, both inorganic and organic in their nature.

In Europe the source of energy in the early days of industrial development was present in a fairly well-developed coal-mining industry in England and Belgium. Iron ore was available in the mountains of Europe in quantity and quality for the fabrication of pig iron and, later, steel. Higher grades of iron and steel have long been manufactured in the Scandinavian peninsula, where wood was an abundant resource and made ample supplies of charcoal available. These fundamental ingredients together made the rise of organized and integrated industry in Europe a possibility.

In North America matters took only a slightly different course. Abundant wood in the East made it a satisfactory power fuel for a long period after coal had become the standard power fuel in Europe. As a matter of fact, wood powered the first steam-operated factories and the first steam locomotives in America. In 1814 our total coal production was reported as 20 long tons. It is a matter of historical record that the cutting of the forests for fuel created the serious local shortages in New England and Middle Atlantic states that were reported about 1820. Need of charcoal for iron caused the harvesting and depletion of vast areas of oak in the central hardwood region until the advent of the Bessemer furnace. From 1830 to 1860, power requirements far outran wood as a supply of fuel, and coal entered upon its continuing role as an industrial fuel.

Thus coal and iron ore became the principal factors in that rapid transformation from handicraft industry to the factory system that characterized most of the nineteenth century. It is of importance to note that neither is obtained in great quantity without the service of wood. Except for the limited amount mined from near the surface in open pits, all coal comes from mine tunnels supported by wooden props: "pit props" in Europe, "mine timbers" in America—a lowly and humble service, indeed, performed by wood to help produce the material that long ago displaced it as an industrial fuel. The volume of wood required for this purpose is considerable. For example, England, in 1947, required nearly 91 million board feet of pit props, of which she imported well over one third. Finland, Sweden, and Norway, rich in

wood but poor in coal, export pit props and import coal.

New basic raw materials and new systems of manufacture brought about new and greatly increased needs in transportation. Highly developed transportation systems for the functioning of coordinated industrial setups all over the world arose in the form of railroads that run on wooden crossties, or "sleepers," as they are called in Europe. In the United States today more than a billion wooden crossties are in use, enabling the rapid transportation of the millions of tons of raw and finished goods that constitute the supply and product of our complex factory system. It takes about 100 crossties per mile per year to service the 420,000 miles of railroad trackage in the United States. Within the past fifty years, the length of service of railroad ties has been trebled by impregnation with coal-tar creosote, a by-product of the industrial system itself, based on coal.

Even the tracks upon which the coal is hauled from the mine, or the iron ore from the pit or shaft, the slides within which the hoists operate, the handles of the tools of the laborers, all are made of wood.

Wood is a necessity in the production of the raw materials of industry and in the transportation systems weaving industry together. The web of communication systems that bind industrial centers throughout the world is mostly carried on wood. In the U.S.A., 90 million miles of telegraph and telephone wire, half of it carried above the ground on wooden poles, are an indispensable part of the industrial setup.

The transformations that came upon the Western world with the advent of organized, integrated industry had nowhere more drastic effect than on the relations existing between urban and agricultural populations. At the time of the beginning of the industrial revolution, the world's population may be said to have been mostly occupied with the business of feeding itself by the application of comparatively primitive tools and methods to agriculture. The advent of industrial production based on steel not only made necessary and possible the development of large centers of industrial population drawn from hitherto rural populations; it made possible, also, great increases in the ability of the agricultural worker to produce food on the farm by supplying him more efficient implements and machinery.

Briefly, the concentrations of population in industrial centers that were required as the system of industrial production developed could not have occurred unless members of rural communities could have been spared from the production of food. Nor could they have been fed had not the productivity per man-unit on the farm been greatly increased. The new agricultural lands of North America literally fed the developing industrial centers of western Europe during their formative years. Industrial Europe still depends to a very large degree upon imported food for its existence. Industrial centers in America depend upon the rapid transportation of food supplies from our agricultural regions for their support.

The volume of food production necessary for the maintenance of the rapidly increasing industrial population was largely obtained as a result of the expansion of agricultural lands in America. This process took place at a tremendous rate and consumed huge amounts of wood for housing, fencing, and farm structures. The two processes of expansion of needs for urban housing and rural housing went on together, and their requirements were met partly by the clearing of the virgin forests from the needed land itself. This process in the United States reached its peak about 1910.

The ratio of this shift of population proportions is well expressed in the statement that, in an industrial country, about one seventh the man power is now required to produce a bushel of wheat that was required a hundred years ago. The tools with which agriculture has become more efficient in the production of food, thus making heavy industrial concentrations possible, have been in turn the product of organized and integrated industry. Wood catalyzed the beginnings of industrial manufacture, serving as a crude beginning material. It continued to serve during the development of the industrial system, and its availability and usefulness were greatly enhanced by the application of the steel tools of production and manufacture that were in turn the product of industry itself.

The pattern of development of an industrial center was almost uniform. First, there came temporary industrial structures and residences, mostly of wood. As technology and markets developed, and permanence for an industrial organization became assured, building took on more lasting character. Industrial structures of steel and concrete and brick became standard, but by far the major part of the dwelling house was still of wood, and wood was even used in the erection of steel and concrete structures.

The industrial system produces the tools of production in increasing abundance, and its products exert great influence upon the continually changing characteristics of the system itself. Steam and steel catalyzed by wood made possible the indus-

trial production of petroleum. There could have been no development of any consequence of the internal-combustion engine until petroleum had become an available industrial raw material in some quantity. Nor could the internal-combustion engine have been of any importance without the availability of huge quantities of steel. Thus the secondary revolution created by the advent and widespread adoption of the internal-combustion engine may be considered to have been a product of the fundamentals of the industrial revolution itself. Similar reasoning can be applied to the remarkable increase in the use of electrical energy. Its large-scale production would not have been possible without the large-scale production of steel and copper, which in themselves are major products of the industrial system.

APPLICATION OF TOOLS AND TECHNIQUES

Briefly sketched, this is the history of the rise of the industrial world. Since a principal characteristic of the system has been and continues to be the application of new tools and techniques to the transformation of materials, it is well to examine the ways in which industry has learned to modify the form and character of wood, the most abundant renewable resource available to it. The industrial system started with wood and, as it developed, increased the requirements for wood to an enormous extent. These requirements have been met by using the new tools of industry in the harvesting and processing of wood.

There are three general ways in which wood enters into use:

1. Through its use as wood itself, its physical form alone being modified to meet diverse requirements.
2. Through improvement of the physical properties of wood by various treatments while preserving its fundamental structure.
3. Through the use of wood as a raw material in the chemical engineering industry.

In the first category one need only examine the woodworking tools of a century ago to understand both their usefulness and limitations. Wood generally is porous, light, strong for its weight, easily shaped with simple tools, and possessed of oriented strength properties that are its exclusive possession among materials. Even in the early stages of industrial development, its workability and adaptability made it a preferred material. Harvested from the forest by the ax and saw, then transported as logs by sled, wagon, and oxen, and, finally, laboriously sawed into boards by hand or slow, water-powered, vertical saws, or even riven into boards by the maul and froe, the shaped pieces of raw wood filled a myriad of uses in a changing world.

But the products of industry in the form of new, tough alloys and machine tools worked a subrevolution in the methods of harvesting and processing raw wood. The rapidly expanding need for lumber to house industrial populations could not have been satisfied except by the application of the tools produced by industry itself. The major equipment of modern logging is steel, from the blade of the ax of the faller to the tractor and arch and the huge trucks that transport the logs to market. Chain saws of new alloys, powered by light gasoline motors, have removed much of the drudgery of logging and greatly increased the production of logs per man-day of labor. In the mills high-speed band saws of tough steel cut boards at an incredible rate. The products of the steel and petroleum industries have revolutionized the lumber-producing industry, and made possible the production of the astronomical quantities of wood required to maintain the industrial system.

In the further shaping of raw wood to its multitude of uses, the employment of high-speed machine tools and assembly-line production makes possible a large and ceaseless flow of houses, furniture, and instruments of modern life that could only take place in a thoroughly mechanized industrial world.

Naturally, an important result of man's ability to harvest and process trees so rapidly and, withal, so wastefully has been the rapid disappearance of the virgin forests that characterized the preindustrial world. The prodigal use of our own forests that has characterized our national life during the past century is without parallel in the world's history. Not only have we become terribly efficient in harvesting and manufacturing wood without regard for the forest, we have also become terribly efficient in clearing land for agricultural use at the expense of the forest, and too often at the expense of the land resource itself. A balance in this matter appears to be in process of achievement; we shall probably keep most of the land now in forests growing trees simply because we shall need the wood to maintain our standard of living. But we will exhaust our virgin forests and be dependent upon smaller logs that we grow for our raw material. We must satisfy the apparently endless demands for wood from forests managed for permanent wood production on land best suited for forest growth.

This does not present insuperable difficulties because, again, the products of industry make possible the efficient use of smaller pieces of wood to do the jobs formerly done by the huge pieces of

structural timber cut from the virgin forests. We have learned, for example, to fabricate ingenious fasteners of steel in order to make strong joints between relatively small pieces of wood, and to construct intricate engineering structures of small wooden members joined by steel.

The advent of synthetic resins, products of an organic chemical industry founded upon the distillation of coal, makes possible the construction of glued forms of wood that were not possible before the industrial revolution. Thus, very large beams, pillars, and arches are built up by laminating and gluing thin boards and shaping them to fit their final use. The glued joints are weatherproof. Because of the vast storehouse of knowledge of the mechanical properties of various commercial species of wood accumulated in recent years, wood takes its place as a precise engineering material and lends itself well to mass production of accurately designed structures.

It is a far cry from the laboriously applied artistic veneers of the eighteenth century to the mass-produced, resin-bonded plywood of today. Plywood is a thoroughly modern product of an industrial world. The long, continuous sheets of thin veneer whirling from a huge log being rotated against a steel knife offer as concrete an example as possible of the application of modern tools to a primitive material. The continuous veneer driers, the automatic glue spreaders, the huge presses, all attest the revolution in the usefulness of wood that has come from the application of the tools of industry.

To make the transformation more dramatic, in recent years we are witnessing the application of high-frequency electric fields to the setting of resin glues on wooden joints and surfaces, thus making possible the production of thicker and more intricate forms of shaped and molded laminated wood products, and continuous line production of boards glued up from small pieces.

In the second category of the uses of wood, the fundamental fibrous and oriented structure of the material is preserved while improving its properties and usefulness.

Wood has limitations in use. When moist, it is subject to attack by fungi and insects. In some uses, therefore, it becomes necessary to impregnate it with materials that are toxic to these attacking organisms. Again, the products of the chemical industry of the industrial world have gone far toward removing this limitation and enhancing the usefulness of wood. Because of their porous structure most woods can be easily impregnated with preservative materials, and, for poles, piling, posts, crossties, crossarms, permanent mine timbers, and exposed structural members, this method is in wide use and is the basis of a large, world-wide industry.

Wood is also combustible and, when carelessly used in construction, can constitute a serious fire hazard. It has been amply demonstrated, however, that by impregnating wood with fireproofing chemicals and using such fireproof wood in critical locations the threat of fire, especially in dwellings, can be effectively minimized. It is of importance to note also that modern laminated beams and arches can withstand very high charring temperatures without serious loss of load-bearing strength, whereas metallic structures subjected to the same treatment often soften and crumple.

We often hear that we live in an age of plastics, but we hardly realize that we have always lived in the age of wood and that wood itself is a plastic. Essentially, wood consists of hollow cellulose tubes, impregnated and bound together by lignin. Under proper conditions of moisture and temperature, these bonds soften and flow so that wood can be molded and shaped. Various forms of molded plywood exemplify the principle.

But, in addition, wood is quite compressible. Its volume is roughly two thirds air and one third solid wood substance. When heated and compressed, many relatively soft and light woods can be compressed to one third their original volume, in which state their specific gravity is trebled, and their strength properties correspondingly increased. Such compressed woods absorb moisture extremely slowly and offer considerable promise in specialized fields of use.

If the wood is impregnated with the components of various of the synthetic resins and then subjected to heat and pressure, there are obtained very hard, dense products, practically completely resistant to the absorption of water and of uniform composition throughout. Such materials are new to the industrial world, so new that their range of usefulness and economics has not yet been well explored.

Thus the products of the chemical industry combine with this age-old material, wood, to give new and more useful forms. But what of the use of wood as a raw material of the chemical industry itself?

Wood may well be considered as the most primitive of the materials of crude chemical engineering, for the beginnings of the manufacture of charcoal for fuel are lost in the mists of antiquity. This was an archaic process, of course, but it produced the

fuel for the primitive extraction and working of metals and the handicraft industries. With the advent of the industrial age, and the possibility of fabricating large shapes of metal, the dry distillation of wood assumed industrial importance and the volatile products, tars, acids, and wood alcohol, were recovered as chemical raw materials. Although recent advances in synthetic organic and fermentation chemistry have greatly lessened the relative importance of wood distillation, it still operates as an important industry, especially on hardwood species. We are still dependent upon charcoal for the manufacture of carbon disulfide, for example, an indispensable substance in the preparation of viscose from wood pulp.

It is considered possible by some that industrialization of certain tropical regions, poor in coal and oil, can be based on dry distillation of the abundant hardwoods of the tropical forest. Naturally, there are difficult unsolved technical problems, but it is not likely that these tremendous renewable sources of carbon and its derivatives will long remain unexploited.

It is an interesting historical fact that a principal source of alkali in preindustrial times was wood; the potash of earlier days was leached from wood ashes, and the slow accumulation of many years of tree growth became the base for the earliest soaps and of wide application in handicraft. Naturally, the industrial age itself early began to produce the so-called heavy chemicals in mass quantities, and it was not long until these chemicals were turned to reaction upon wood itself for the production of wood pulp and paper.

The paper-making industry is very old, indeed, and its ancient materials were the agricultural fibers and certain grasses. With the advent of the combination of industrial chemicals and wood, however, a deep transformation in character and magnitude took place, so that paper from wood in huge volume became a leading characteristic of modern civilization. The influence of cheap and abundant paper upon the course of modern society can hardly be estimated and is worthy of volumes of discussion in itself.

Nor is the end of development of the usefulness of defiberized wood in sight. Whether we speak of chemically or mechanically prepared wood fibers, the array of new products and new processes that appear annually assumes staggering proportions. At the one extreme are the simpler wood fibers prepared by the disintegration of wood by mechanical or mild chemical means and their reassembly in new physical form. Some of the most interesting of these take the form of sheets of lightly compressed fibers sandwiched between thin, hard sheets of other materials such as metals, fiber glass, or fabrics sometimes impregnated with synthetic resins to give a bewildering variety of new materials. Or the low-density core may be glued between thin sheets of wood veneers to produce light, strong materials of new beauty and usefulness.

A striking, recently developed material consists of paper sheets, impregnated with synthetic resins, or perhaps with the lignin residue from the pulping of the wood itself. The impregnated sheets, piled to the desired thickness, are subjected to heat and pressure and emerge as a tough, strong material, approaching mild steels in properties. They have a singular advantage, however. Because of their fundamentally fibrous structure, and because the strength properties of the final product can be varied by the orientation of the fibers, it is possible within limits to construct the material to meet specialized requirements of use.

Similarly, as an infinity of specialized pulping processes and pulp treatments has developed, a marvelous variation in the properties of paper for a wide field of uses has been attained. Tough, strong paper for packaging has a far different origin and process than a delicate facial tissue. Book papers and newsprint arise from different woods and different manufacturing processes. And, again, the combination of paper with the new resinous products of the chemical industry is yielding papers with high strength properties, even when wet, for a vast new field of usefulness. It is interesting to speculate upon what the course of World War II might have been but for the packaging of goods in paper and paperboard.

Much of this progress has grown out of our expanding knowledge of the basic nature of the wood fiber itself and the fundamental chemistry of the cellulose molecule. It must be considered that this field of scientific endeavor, a part of the large field of the chemistry of high polymers, has only within very recent years reached a stage in which one may speak of cellulose molecules and chains with some degree of accurate meaning. And it is only still more recently that meaning has been given to the term "reactivity" as applied to cellulose. Even during the developmental period, we have witnessed the advent of new fibers and new resin and lacquer components based on cellulose that required the production of alpha-cellulose of high purity in industrial quantities. Soon, with developing knowledge of the fundamental reasons for the variable properties of industrial cellulose, it will be possible to specify not only the content of alpha-cellulose, but the average chain

length and limits between which chain lengths may vary and the specific reactivity of the cellulose chains under definite conditions. These requirements for highly specialized properties in wood cellulose are now known to be characteristics of not only its method of preparation, but of the manner of its formation in the living wood. In the future, we will select species for special uses, and give them even more highly specialized methods of cellulose extraction.

Since industrialization has taken form largely in the north temperate countries, in which the major forests were coniferous, it is not surprising that most development of the paper and cellulose industries has been based upon coniferous woods. Increasingly, the needs for paper and cellulose that accompany the growth of integrated industry have gone beyond local supplies of coniferous wood and into zones of essentially broad-leaved or hardwood forests. We shall, therefore, witness increasing use of these species for cellulose and paper and wood fiber products.

In chemical processes for the production of cellulose, roughly one third the weight of the wood goes into solution as that substance or mixture of substances known as lignin. There are few materials that have been subjected to more scientific investigation than lignin, and yet, at a recent meeting of lignin chemists in this country, the first and last questions posed to the assembly were concerned with the nature of lignin.

The difficulties attendant upon the characterization of lignin arise from the fact that, in the wood itself, it is probably combined with cellulose and the hemicellulose, and in the processes of isolation it undergoes deep-seated changes that vary quite broadly according to the methods used. Thus the term "lignin" actually has no specific reference apart from complete description of the methods used in its preparation. To make matters still more difficult, hardwood and softwood lignins differ, and it is quite probable that the ultimate composition of lignin may vary from species to species.

In spite of the problems posed by this infinity of variables, considerable progress is being made in the industrial utilization of those lignins that are the by-products of the acid and alkaline processes for the preparation of wood pulp. The alkaline industries generally burn the evaporated lignin containing liquors for power in the plant, but, increasingly, the lignin is being precipitated and recovered by various means for a wide variety of uses as crude lignin.

The sulfite pulping industry is faced with another by-product of the industrialization of the world, namely, stream pollution. Difficulties in evaporating and consuming its waste liquors have caused it to be attacked as a major contributor to this grave problem. The pollution with industrial wastes of the water supplies of industrial communities cannot much longer be tolerated in a civilized world. Large quantities of used industrial water must not be discharged in such a manner as to endanger the lives and health of people. Because of the growing importance of limited water supplies, rapid progress is being made in processes for the checking of stream pollution from sulfite pulp mills. It is clear that profitable utilization of the lignin component of wood is the key to preventing stream pollution from this source.

In acid processes for the pulping of wood, notably the sulfite process, considerable amounts of sugars are liberated and are present in the waste liquors discharged. In those countries not blessed with the abundance of foodstuffs available in this country, these sugars are thoroughly utilized, usually by fermentation. In Sweden, for example, the fermentation of these sugars to alcohol or other products is required. Large quantities of yeast for human or animal food are prepared from the same raw material.

In North America, two sulfite pulp mills now convert the hexose content of their waste liquors to ethyl alcohol, and a third plant is under construction. Pilot-plant operations on the production of feeding yeast from the complete sugar content of the liquors have been successfully completed, and it is expected that commercial production of sulfite liquor yeast will soon be established.

Important as are these developments in the pulp industry, their ultimate magnitude must be strictly limited by the size of the sulfite pulp industry. This in turn is distinctly limited in the species of wood that it can use, and it can reach only the limit set for it by the capacity of forest lands to produce those species.

FUTURE DEVELOPMENT OF SUPPLIES

From the considerable number of industries based upon wood that have already been described as part of the industrial world, it would appear possible to utilize rather completely the products of the forest. In some parts of the world such utilization is possible and practiced, but for the most part the forest industries have been and remain prodigal in their consumption of the forest crop by one-product industries, highly selective of the species and qualities of wood manufactured. Yet in recent years it has become increasingly

apparent that only the highest possible degree of integration of wood-using industries can bring about the necessary balanced utilization of the forest crop required for the perpetuation of the crop itself. The forest is unique among all other crops of the soil in this respect: The method of the harvest determines to a large degree the nature and value of the succeeding tree crop. It is for this reason that wood industries, widely differentiated so as to use the forest crop completely, and integrated with the capacity of tributary lands to produce wood, have become considered necessities for the future development of supplies of wood adequate to our need. The principal lack in the ability of the industrial world to establish such completely integrated industrial organizations has been the absence of a type of wood-using industry capable of taking a wide range of species and qualities of wood and economically manufacturing from this raw material goods required by the modern world in mass quantities.

When it is considered that the sawing of wood into lumber brings only roughly one third of the total yield of the forest to market in the form of usable material and that a similar proportion holds true in the field of pulp and paper products, the significance of such an industry capable of profitably utilizing the other two thirds of the wood crop becomes apparent. As indicated before, there may be situations, especially in tropical countries, where the carbonization of wood and the consumption of the carbon products as such or for conversion to liquid fuels may offer a solution to this problem. The simple consideration of the fact that a large part of the fuel value of wood resides in the lignin, and that all carbohydrate content of the wood is destroyed in the process of carbonization, leads to the idea that the recovery of the carbohydrates in the form of simple sugars prior to carbonization of the lignin content might be a better solution to the problem posed than total carbonization.

Considerable progress has been made in this field along the line of acid hydrolysis of the carbohydrate content of wood. Although it has been more than 125 years since it was first ascertained that cellulose could be broken down by acids to glucose molecules, it has only been in the twentieth century that serious attempts to base industries upon this simple fact have begun to show some promise of success. A wide variety of hydrolytic agents has been employed, ranging from quite dilute mineral acids to extremely concentrated mineral acids; a considerable variation of temperature and pressure has also been employed. Suffice it to say that the technological means for the conversion of the carbohydrate content of wood to simple sugars are available and that by known processes approximately half the weight of the raw wood can be obtained in the form of simple sugars. In the case of coniferous woods, approximately 80 percent of these sugars is glucose and the remainder a mixture of pentoses, mostly xylose. In the case of the hardwoods, the proportion of pentoses is increased to around 35 percent. No great difficulties are encountered in the use of the crude sugar solution obtained by these processes of hydrolysis as the base for various fermentation industries with the production of industrial chemicals as the aim. Under current conditions no difficulty should be experienced in maintaining profitable production of such materials in competition with other sources of carbohydrates or even with petroleum for industrial chemicals. There are many serious considerations, however, that should affect our thought on the use of wood as a source of carbohydrates, other than the mere production of industrial chemicals.

It has been remarked earlier that one of the accompanying features of the rise of industrialization has been a tremendous increase in the population of the world. In a little over a century the world's population has increased by approximately two and one-half times. Fairfield Osborn in his striking book, *Our Plundered Planet,* estimates that there are not more than 4 billion acres of arable land left to fill the needs of 2 billion people, or less than 2 acres per capita. A generally accepted computation has it that the products from 2.5 acres of land of average quality are required to provide even a minimum, adequate diet for each person. These figures indicate the heavy impact of population on the agricultural resources of the earth when those resources are measured in terms of traditional methods for the production of food. Further industrialization of agriculture and the possible exploitation of new lands can only delay a little the application of the ironclad rule of Dr. Malthus. Tropical regions cannot be depended upon for agricultural productivity, and even if they could would only provide a temporary stopgap. Furthermore, there can be no political stability in the world if the basic needs of the people for subsistence are not satisfied. The most important things in the modern world, whether they be weapons of war or peace, are food and machinery. Both are today products of an industrial world, but it would be well to examine especially the means by which the industrial facilities of which we are possessed can be better turned to the satisfaction of the world's needs for food.

If we consider the unused wood crop in the United States alone, it amounts to something in the neighborhood of 150 million tons a year. This is a very conservative figure and has little relationship to the amount of wood that could be grown on the 630 million acres of forest land available in the United States if this land were properly managed. There are no really reliable figures on the amount of the wood crop that could be produced in the world under even simple forms of forest management. Some measure of the potentialities may be obtained from the estimate recently made to the effect that the energy stored by photosynthesis in a year is approximately 100 times all the coal mined per year, or 10,000 times the energy at present being obtained from water power. What proportion of this total is stored in the form of wood cannot be estimated, but it certainly is large.

The question that needs to be solved is the turning of this vast quantity of renewable carbohydrate material, at least in part, to the task of feeding the world's population. It has already been stated that methods for the conversion of the carbohydrate content of wood to simple sugars are available. By the addition of nitrogen, extracted from the air by known industrial methods, and small amounts of other nutrient materials amply available from industrial processes, the conversion of the crude sugars, produced by the hydrolysis of wood, to protein is easily effected. Roughly, a ton of wood can be converted into a quarter ton of yeast with a protein content of approximately 50 percent. When it is considered that only a few of the many species of microorganisms have ever been grown for such purposes, the opportunities for further profitable investigation in this field become striking. We have here the possibility not only of the production of vast quantities of edible carbohydrate material from wood, but also their transformation into nutritive protein, which is perhaps the greatest single lack in the diet of most of the world's teeming population. It has been estimated that if it became necessary the total sugar requirements for the population of the United States could be currently obtained from the sawmill waste now being produced in the states of Oregon and Washington.

When one considers the availability of carbohydrate materials from wood in the tropics, the potentialities of this raw resource in the production of foodstuffs are indeed staggering. The forests of the tropics do not apparently offer unlimited wood to replace or supplement the traditional uses to which wood in the Northern Hemisphere has been put. But there is a vast and unknown storehouse of carbohydrate material awaiting development.

The successful, complete wood chemical industry will not be a unit in itself. It will be geared to the forest and to such other wood-utilizing industries as may be found suitable to the kinds and qualities of wood that the forest grows. There are three characteristics that distinguish wood as a raw material for chemical industries:

1. It is a perpetually renewable resource of the same kind and in the same place.
2. Wood for chemical industries can be a by-product of forestry of which the principal product may be lumber or similar material.
3. The methods used in harvesting the wood crop determine the nature of future crops.

The industrial plants that utilize the product of the forest determine the harvesting methods.

Diversified utilization of the forest crop can be expected to encourage intensive management of the forest. As long as the sun shines and the rains fall there need be no shortage of wood in the world if we manage the forest well and learn to use the tools of industry, products of our industrial civilization, for the more complete employment of wood in meeting human needs. And, above all, the land that grows the forest is land that usually cannot be put to other profitable use. The forest resource is unique in that its proper harvesting is the best means possible of ensuring a permanent forest resource.

DEFINITIONS AND PRINCIPLES OF DYNAMICS*

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IN A recent paper[1] the development of the exposition of thermodynamics was traced from its beginnings to the present. It was pointed out there that more than half a century elapsed between the time the first law of thermodynamics was understood sufficiently well for effective application by savants like Kelvin and Clausius and the time when an adequate exposition was first published. It was found that the causes of this long delay included the following: first, a vestige of the caloric theory to the effect that heat resides in a body and is a state property: second, the persistence of the idea that nothing is understood unless it can be explained in terms of Newtonian mechanics.

This inquiry into the foundations of thermodynamics brought into focus two points—first, that the exposition hinged on definitions of a few quantities, the definitions consisting of rules for getting numbers to represent the magnitudes of quantities designated by certain names; second, that these quantities antedated in the minds of men the new science of thermodynamics.

Let us review briefly the exposition of the first law of thermodynamics, following the method of Henri Poincaré, the first adequate method. The first law is the statement that work and heat are proportional to each other in a cyclic process. It remains to define the terms work, heat, and cyclic process. Beginning with the last we say that a cyclic process is any process in a prescribed system (or collection of matter) for which all observable characteristics of the system are the same initially and finally. Work and heat are both quantitative effects of one system on another. We shall discuss each separately.

Work flows from system A and to its environment during a given process in A if A could pass through the same process while the sole effect external to A was the rise of a weight. The number which represents the magnitude of the work is found by counting the number of standard weights which can be raised by A from one selected level to another at a given location on the surface of the earth. The amount of work flowing from system A is identical with the amount flowing to its environment. The implication here that work is always measured in terms of flow *from* a system is a necessary one.

The definition of heat involves a preliminary definition of temperature which in turn requires a previous definition of equality of temperature. Any pair of systems, in the absence of any extraordinary barrier between them, will in general affect each other even in the absence of work. The corresponding changes in two systems that are exposed only to each other will gradually decrease in rate. The condition which the pair approaches as a limit is called equality of temperature.

Associated with this definition is an axiom, sometimes called the zeroth law of thermodynamics, to the effect that two systems that are each equal in temperature to a third system are equal in temperature to each other.

A temperature scale may be defined in terms of certain reproducible levels of temperature and the magnitude of a selected property of a prescribed system under prescribed restraints as to pressure, volume, etc.

The definition of equality of temperature implies the existence of inequality of temperature, for which the two systems in communication have an effect on each other. This effect is called heat. The number which represents the magnitude of the heat for a process of system A is found by counting the number of standard systems in communication with A which will go from one prescribed level of temperature to another as a result of the heat effect in question. For the purpose of this test system, A must exchange heat during the process only with the standard systems which are subject to prescribed restraints as to pressure, volume, etc.

These rules illustrate the two points mentioned above. First, they are rather elaborate rules for counting to get numbers to represent the mag-

* From a lecture presented by Professor Keenan at Purdue University, April 27, 1948.

nitudes of work and heat. Second, they are new rules for old quantities. Work is a quantity appearing in the early discussions of mechanical equilibrium or statics. In a displacement of a balanced lever the work quantities at the two ends were known to be equal in magnitude and opposite in sign. Work had previously been associated with a force. Here it is an interaction between systems. Heat was variously considered as an interaction between bodies and as something stored in bodies. Calorimetry, or the measurement of heat, was not new but had not previously been strictly defined.

The new principle—namely, the first law—is stated in terms of these old concepts redefined.

Upon closer examination of these definitions three additional points become clear: first, the new principle results in more precise definition of the old quantities; second, these precise definitions, or rules of counting, are strangely arbitrary in nature; and, third, each involves "extrapolated concepts" or "extrapolated devices."

The first of these points has already been commented upon. The second, relating to arbitrariness, becomes evident upon considering the rule about measuring work flowing from a system in order to measure work flowing to another, or the rule about heat effects in an interaction being equal and opposite. These definitions are *ad hoc* devices tailored to the purpose of stating the principle at hand.

As regards extrapolated concepts, we find them implied throughout these definitions. When a gas expands behind a piston the effects outside the gas will include, besides the rise of a weight, the warming of bearing surfaces between piston and cylinder and between links connecting piston and weight. By repeating the experiment with greater and greater refinement of bearings so that less and less material is warmed, a condition is approached, though never attained, for which the sole effect is the rise of a weight. The work for this limiting condition can be obtained only by extrapolation.

As another example, consider the condition in equality of temperature requiring that the two bodies should be isolated from everything but each other. This condition can be attained only by means of an indefinitely thick wall of inert material as the immediate environment of the two bodies. Equality of temperature can thus be found only by an extrapolation of observations to an unattainable condition.

This concept of extrapolated values implies two things: first, that the only limit to the nearness with which we may approach the limiting condition is one imposed by our own limitations as to time, effort, or diligence; second, that results obtained with different materials or devices, within the prescriptions of the definitions, will converge upon each other as the limiting condition is approached.

This approach to the subject of thermodynamics has justified itself in that it has made the subject intelligible to men of ordinary intellect. Whereas before only the savant could safely find his way through the obscurities, confusions, and apparent contradictions of the subject, now many ordinary minds can have the satisfaction of solving problems not solved in the textbooks. Indeed, this success has been so notable that one is impelled to inquire whether similar methods have been applied to the exposition of the sciences of statics, dynamics, electromagnetism, etc.

On this occasion it is proposed to make this inquiry as regards dynamics. It is doubtless a foolhardy thing to do. To venture with critical eye and pen out of one's own specialty into someone else's is a tried and true method of losing friends and alienating people. The present justification for proceeding along this perilous path is the hope that someone better qualified will be inspired, if only through resentment, to clarify this subject.

The subject of dynamics has been critically examined on several previous occasions by scientists of uncommon skill. A reasonably extensive sampling of the literature seems to indicate that a statement of the principles of dynamics of a clarity comparable to Poincaré's statement of the first law of thermodynamics has not yet appeared. Two faults seem common to all published statements: first, that the necessary definitions are not refinements of previously recognized quantities; second, that the definitions do not include practical counting procedures, which through extrapolation will yield the desired numbers.

Just as the exposition of thermodynamics was long hampered by following too slavishly the words of the great pioneers, so too the exposition of dynamics seems to have suffered. Whereas in thermodynamics we had to overcome the handicap of Helmholtz' mechanical theory of heat, in dynamics we seem to suffer from Newton's preoccupation with astronomy. A reading of several eminent authors in this field is likely to leave one with the impression that these men had lived their lives floating about in space—*extra*stellar space—in which they could perform at will experiments quite beyond the capacity of earthly mortals.

Let us first note that the extrapolated concept must enter into each definition in dynamics. As

an example, consider the quantity length, which must certainly be defined. We shall not complicate the matter by considering relative velocities great enough to raise questions of the meaning of simultaneity, but shall limit ourselves to a prerelativity concept of length. We may prescribe the distance between two marks on a particular bar of metal held at a particular temperature as the unit of length. When the bar is moved in the direction of its length to a position where the first mark is where the second was previously, then we have added a second unit of length to the first to obtain two units of length. The question arises whether the orientation of the bar is of any consequence. The answer seems to be in the affirmative; that is, two bars which give the same number for a horizontal distance may give slightly different numbers for a vertical distance.

Upon investigation it is found that bars which for a given weight have great resistance to deformation will show better agreement in measurement of vertical distances than bars having small resistance to deformation. Here we must extrapolate the result to that for a measuring bar of infinite rigidity per unit weight.

To avoid this difficulty, as well as some others, it is now the practice to use the wave length of a certain part of the electromagnetic spectrum as the basic distance in terms of which the unit of length is defined. Nevertheless, extrapolation of the result of counting to idealized conditions is still necessary.

Newton, like Kelvin and Clausius in thermodynamics, had an intuitive grasp of the significance of his discoveries. He did not present them in terms adequate to the understanding of the common mind, nor even with any logical completeness. He defines several different kinds of force, one of which, "an impressed force, is any action upon a body which changes or tends to change, its state of rest, or of uniform motion in a straight line." When this is compared with his first law, namely, "Every body perseveres in its state of rest or of uniform motion in a straight line, except in so far as it is compelled to change that state by impressed forces," definition and law become indistinguishable.

Ernst Mach in 1883[2] reviewed Newton's exposition critically and came to the conclusion that "In reality only *one* great fact was established [by Newton and his scientific forbears Galileo and Huygens]. Different pairs of bodies determine independently of each other, and mutually, in themselves, pairs of accelerations, whose terms exhibit a constant ratio, the criterion and characteristic *of the pair.*" Although a good argument can be made that Newton's principles embrace more than this, it is significant that a careful student of the subject could not convince himself from Newton's exposition that Newton referred to anything more than a ratio of a pair of accelerations for a pair of bodies.

Let us see what Mach proposed in place of the exposition of Newton.

> It is possible to replace Newton's enunciation by much more simple, methodically better arranged, and more satisfactory propositions. Such, in our estimation, would be the following:
>
> a. *Experimental Proposition.* Bodies set opposite each other induce in each other, under certain circumstances to be specified by experimental physics, contrary *accelerations* in the direction of their line of junction.
>
> b. *Definition.* The mass-ratio of any two bodies is the negative inverse ratio of the mutually induced accelerations of those bodies.
>
> c. *Experimental Proposition.* The mass ratios of bodies are independent of the character of the physical states (of the bodies) that condition the mutual accelerations produced, be those states electrical, magnetic, or what not; and they remain, moreover, the same whether they are mediately or immediately arrived at.
>
> d. *Experimental Proposition.* The accelerations which any number of bodies A, B, C . . . induce in a body K, are independent of each other. (The principle of the parallelogram of forces follows immediately from this.)
>
> e. *Definition.* Moving force is the product of the mass value of the body into the acceleration induced in that body.

Of these, the Experimental Propositions *c* and *d* cannot well be objected to. Proposition *c* states that the mass ratio of two bodies is independent of the physical states of the bodies. Proposition *d* states the independence of force effects from each other and indicates vector summation of forces and their effects.

Experimental Proposition *a,* on the other hand, has a disturbing vagueness. "Bodies set opposite each other induce in each other, under certain circumstances to be specified by experimental physics, contrary accelerations. . . ." Quite obviously the definition *b,* which is to the effect that the mass ratio of two bodies is given by the ratio of accelerations, is only valid for bodies which are acted on only by each other or for bodies constrained to move in a horizontal plane but otherwise acted upon only by each other. This latter is so special an interpretation as to seem not to be Mach's intention.

It is the misfortune, perhaps, of earthbound creatures that they can never experiment at will upon two bodies acted on only by each other. The dynamicist's lament might well be "The earth is too much with us; late and soon," which is only a slight variation on Wordsworth's. As a terrestrial proposition *a* is not experimental.

In thermodynamics we have learned to be terrestrial in our outlook, to the great advantage of the student. Gibbs opened his great paper "On the Equilibrium of Heterogeneous Substances" by quoting Clausius: "The energy of the Universe remains constant, the entropy increases toward a maximum." Some of his followers have stated that these were the principles from which Gibbs' argument proceeded. On the contrary, they played no part in his argument but served only to embellish his first page. Such cosmic principles are of no utility in laboratory or workshop. The universe cannot be the subject of controlled experiment.

A further objection to these statements of Mach is that neither mass nor force, as here defined, is a concept known aside from these definitions. This despite the fact that both mass and force were well-recognized concepts before the advent of dynamics. Mass was a concept of the market place; force was a concept of the workshop and of the science of statics.

Max Planck, writing in 1916,[3] expounded the fundamentals of dynamics by steps which may be roughly outlined as follows:

"A material point which is deprived of all causes of motion moves uniformly and rectilinearly." This may be illustrated, but not proved, by the motion of a body on a smooth horizontal surface.

Muscular effort is required to accelerate a body. For a given body the effort increases with the acceleration. Therefore, we partially define a force by saying that it is something acting on a body which is proportional to the acceleration of that body.

But some bodies require more muscular effort than others for the same acceleration. Therefore, the proportionality constant between force and acceleration must depend upon some characteristic of the body, which we shall call mass.

Up to this point it should be noted that neither force nor mass is completely defined.

It is known from experiment that all bodies have the same acceleration when falling freely. Therefore the force causing the fall, or gravity, is proportional to the mass.

Now, if equal masses are attached to the two ends of a string which hangs over a pulley, the forces on the two ends will be equal and the string will not move from rest. Thus we have a means of measuring masses.

Force now becomes a derived quantity, namely, the product of mass and acceleration.

Although this presentation seems more realistic than that of Mach, it leaves one unsatisfied in several respects.

First, the reasoning is involuted. An inadequately defined force is proportional to an inadequately defined mass for fixed acceleration.

Second, the concept of force is not identified with the concept of force in statics. How can we be sure that equal forces of the kind in question will not rotate the pulley? Is not the pulley device an arbitrarily selected means of measuring mass, rather than a means derived from the previous considerations? By inference we may find an association of the two concepts, static force and dynamic force, through reference to muscular sensation—a reference not made by Mach.

Both Mach and Planck define force as if it were a concept which derives from the dynamic principle, although Planck does attach to it a qualitative "muscular" significance as well. In view of these definitions, it is fair to conclude that force is another name for the product of mass and acceleration. All forces in statics are omitted from this definition. It becomes logical to assume that to support a body at rest involves no force because it involves no acceleration. Perhaps the forces on the string over the pulley in Planck's measurement of mass vanish when the bodies are brought to rest. Here is no recognition of the back-breaking toil of generations of laborers who had to "lift dat load and tote dat bale." Lifting and toting was the serious work of man—and the worker soon learned to leave acceleration to those foolish enough to waste their substance on athletics.

It might be argued that the definition in terms of mass and acceleration will do if forces can be applied one at a time, so that counteracting forces may be recognized. But this argument seems meaningless in the absence of further means of recognizing forces. Force is evidenced by acceleration and alters along with acceleration. When acceleration vanishes, force vanishes; whether from cancellation of one force by another or by the vanishing of all forces, we cannot determine.

A. G. Webster, in his *Dynamics of Particles* (1912),[4] also defines force as the product of mass and acceleration. He seems to try to break through the difficulty just discussed.

The introduction of the term force has given us no explanation of the cause of motion. . . . It is undoubtedly true that our fundamental notions of dynamics are derived through what may be called the muscular sense. . . . Suppose that we find that under given conditions a certain agency will produce a certain force, as shown by the motion of some body, and suppose that as the circumstances are changed we can always measure the force. If then it is possible to submit a second body to the action of the same agent under similarly varying circumstances, we shall be able to find the motion of the second body. . . . Under certain circumstances, an agent which would under other conditions cause motion, may cause no motion. We then say that its effect is counteracted by that of some other agent, or otherwise, that the two forces are in equilibrium.

Here the implication is that the existence of a force may be recognized in terms of the agent that provides it, although the implication is vague.

The phrase "to submit a second body to the action of the same agent under similarly varying circumstances" suggests that identical forces may be recognized aside from their accelerating effects, although the means of recognition are not clear.

Planck and Webster both imply that force is a concept in its own right, that it derives from muscular effort, that it may be recognized in terms of the agent that provides it. Webster says it is an interaction between bodies, and Mach states that such interaction exists but only indirectly associates it with the term force. Nevertheless, no one of these authorities suggests that a measure of force can be found other than the product of the acceleration it produces and the mass it acts upon. Force, in the last analysis, is nothing but the product of mass and acceleration, and the principle of dynamics becomes, in part at least, a definition of force.

It seems evident that a principle stated solely in terms of previously defined quantities is simpler than one combined with a definition of a new concept or a redefinition of an old one. In accordance with this view, we state the first law of thermodynamics as *the proportionality of the quantities work and heat,* as previously defined, *in a cyclic process.* Subsequently, we may derive a unit of work or of heat by making the constant of proportionality unity. But to state this law in the form *heat and work are equal in a cyclic process* without first having defined heat, for example, would be to obscure the principle quite hopelessly. If we were to combine such a statement with some vague remarks concerning the physical sensations of warmth associated with heat we should leave our principle about where the principle of dynamics seems now to be left.

Since Poincaré resolved many of our difficulties in the exposition of thermodynamics, it may prove profitable to read what he has to say about the exposition of dynamics. The following is from his *Science and Hypothesis* of 1903, as translated by Halsted.[5]

> The Principle of Inertia—A body acted on by no force can only move uniformly in a straight line. . . .
>
> Is the principle of inertia, which is not an *a priori* truth, therefore an experimental fact? But has anyone ever experimented on bodies withdrawn from the action of every force? And, if so, how was it known that these bodies were subjected to no force?
>
> The Law of Acceleration—The acceleration of a body is equal to the force acting on it divided by its mass. Can this law be verified by experiment? For that it would be necessary to measure the three magnitudes which figure in the nunciation [sic]: acceleration, force, and mass.
>
> I assume that acceleration can be measured, for I pass over the difficulty arising from the measurement of time. But how measure force, or mass? We do not know even what they are.
>
> What is *mass?* According to Newton it is the product of the volume by the density . . . it would be better to say that density is the quotient of the mass by the volume. What is *force?* It is, replies LaGrange, that which moves or tends to move a body. It is, Kirchhoff will say, the product of the mass by the *acceleration.* But then, why not say that the mass is the quotient of the force by the acceleration?
>
> These difficulties are inextricable.
>
> When we say force is the cause of motion, we talk metaphysics, and this definition, if one were content with it, would be absolutely sterile. For a definition to be of any use, it must teach us to *measure* force; moreover that suffices; it is not at all necessary that it teach us what force is *in itself,* nor whether it is the cause or the effect of motion.
>
> We must therefore define the equality of two forces. When shall we say two forces are equal? It is, we are told, when applied to the same mass, they impress upon it the same acceleration, or when, opposed directly one to the other, they produce equilibrium. This definition is only a sham. A force applied to a body cannot be uncoupled to hook it up to another body, as one uncouples a locomotive to attach it to another train. It is therefore impossible to know what acceleration such a force, applied to such a body, would impress upon such another body if it were applied to it. It is impossible to know how two forces which are not directly opposed would act, if they were directly opposed.

In the interest of brevity, the remainder of Poincaré's discussion will be summarized. He attempts to use the downward pull of an arbitrarily selected heavy body for identifying vertical forces, but concludes that this pull is variable with position and that therefore mathematical rigor is lacking. He rejects the resiliency of a dynamometer as a measure of force because "temperature and a multitude of circumstances" may cause it to vary. He points out that "We are . . . obliged in the definition of equality of forces to bring in the principle of equality of action and reaction; on this account, this principle must no longer be regarded as an experimental law, but as a definition."

He discusses the consequences of accepting what he calls Kirchhoff's definition: "force is equal to the mass multiplied by the acceleration." He points out that this, with the principle of action and reaction, results in determining the ratio of a pair of masses by the ratio of their accelerations *when they act upon each other only,* an impossible condition.

He speculates as to using the law of universal gravitation, but seems to reject it, because he questions the right to admit the hypothesis of central forces. He says finally: "Therefore nothing remains and our efforts have been fruitless; we are driven to the following definition, which is an

avowal of powerlessness: *masses are coefficients it is convenient to introduce into calculations.*"

He raises the question whether the principles of dynamics are no more than definition and, in trying to show that they are more than this, says:

> There is not in nature any system *perfectly* isolated, perfectly removed from all external action; but there are systems almost isolated. . . . We ascertain then that the motion of this center of gravity is *almost* rectilinear and uniform, in conformity with Newton's third law.
>
> That is an experimental truth, but it cannot be invalidated by experience . . . a more precise experiment . . . would teach us that the law was only almost true.
>
> *We can now understand how experience has been able to serve as basis for the principles of mechanics and yet will never be able to contradict them.*

It seems that Poincaré leaves us on the horns of the dilemma. Nevertheless, he has stated with great clarity the problems we must face. These are, in brief, as follows:

> No one has ever been able to experiment on a body withdrawn from the action of any force. His later contention that there are systems *almost* isolated must refer to an astronomical mechanics rather than a terrestrial one.
>
> "A force applied to a body cannot be uncoupled to hook it up to another body, as one uncouples a locomotive to attach it to another train." Identical or equal and opposite forces can therefore not be determined.
>
> "For a definition to be of any use it must teach us to measure force; moreover that suffices. . . ."
>
> "Masses are coefficients it is convenient to introduce into calculations."

From the standpoint of terrestrial mechanics, however, one need not despair of resolving these difficulties. One need not despair of devising a definition for mass that will teach us to measure mass, and a definition for force that will teach us to measure force and to uncouple a force and to hook it up to another body. Despite the melancholy implications of the limerick

> An intrepid young student named Raleigh
> Deserted a fast-moving traleigh
> To discover a flaw
> In Newton's first law,
> But his efforts were faleigh, by galeigh,

it is proposed here to embark on an attempt to state a set of definitions and principles fundamental to the science of mechanics in general and of dynamics in particular. The result can hardly be definitive, though it may prove provocative.

The definitions will be primarily rules for counting to obtain numbers which will represent the magnitudes of what we call length, time, mass, and force. These might be called the primary physical quantities. Regarding these A. H. Shapiro has made the following useful comments, which are quoted with his permission:

Primary physical qualities have no "inherent," "intuitive," or "absolute" significance. They are defined arbitrarily, and their definitions consist of sets of specifications for assigning numbers in experiments where the sole ingredients are certain arbitrarily-selected bodies and our own physical senses.

The definitions of primary physical quantities are, for practical reasons, heuristic. The aim is to discover relations or laws between various primary and secondary quantities so that natural events may be described and, ultimately, predicted. The usefulness of the primary quantities, as defined, is judged by (*i*) whether such laws may be found at all, (*ii*) whether the laws as so found are simple to apply, (*iii*) whether the method of assigning numbers to the primary quantities is reasonably simple and essentially free of ambiguity.

To make the definitions of primary quantities unambiguous, the specifications for assigning numbers become complex. In all practical cases, this requires extrapolations (with the implied notion of convergence) for getting numbers accurately.

The quantity length was discussed above in sufficient detail to show that it is manageable, though troublesome. Equally troublesome is the quantity time.

Newton implied that we had to make do with an astronomical time, although he looked wistfully toward an absolute time, otherwise called duration, the flow of which cannot be changed.

Various authorities state that the basic unit of time in mechanics is the "mean solar day." The adjective "mean," however, suggests that solar days are of varying duration, which in turn suggests some measure of time independent of the solar day. This seems to be the sidereal day. Smaller units of time, like the hour, minute, and second, are subdivisions of the mean solar day, which are arrived at by means of clocks. A clock consists of some mechanism which regularly repeats some operation such as the oscillation of a pendulum. Presumably, the acceptability of clocks depends largely on agreement between them as to the subdivision of the mean solar day into fractional units such as the second and the hour. The assumption is implicit in this definition of time that the time elapsed between two events is independent of the position or motion of the observer. A science based on such an assumption can be no more valid, of course, than the assumption itself.

The rule for measuring mass is that the body supported in one pan of an equal-arm balance, which is balanced and free, is equal in mass to that supported in the other. The equal-arm balance must, of course, balance again when the bodies at either end are interchanged. An arbitrary collection of matter may be considered the unit of mass. That and the material which balances it constitute together two units of mass, and so on. Two equal

masses which together balance the unit of mass are each one-half unit of mass, and so on.

This is, in fact, Planck's method of measuring mass written into a definition. The involution of argument employed by Planck is avoided by this arbitrary step.

Force is an interaction between two bodies. (Heat is also an interaction between two bodies, the rules of measurement for which are different from those for force.) When the interaction occurs at the surfaces of the bodies, as when it results from contact, then distortion of the bodies occurs. The amount of the distortion may be different for two different bodies in contact. The body which is distorted less is said to be the more *rigid*. The concept of perfect rigidity, or no distortion under force, is an extrapolated concept.

Some bodies have the characteristic of complete recovery from distortion when the interaction ceases. These are called *elastic* bodies. The distortion of elastic bodies may be used to measure force.

A long elastic body on a horizontal surface has the same length regardless of its orientation, provided that it is not in contact with, or too near, any body other than the surface. As its position is altered from the horizontal some change in length generally occurs, although the amount of this change differs from one body to another. The greater the mass of the body for a given rigidity, the greater will be this effect of departure from the horizontal. For the purpose of measuring forces it would be desirable, although not necessary, to have an elastic body of negligible mass for a given finite rigidity.

For measuring forces in a horizontal plane, one may select a single long, slender elastic body arranged with "grips" at the ends which permit the attachment of strings. A unit of force may be said to act on each end of this body directed away from the body and along the line defined by the stretched string when the body is simply extended by a prescribed amount which is within the range of elastic behavior of the body. The temperature of the body must be prescribed; for example, it may be required that the body be surrounded by an ice bath. The extrapolated nature of the measurement of force becomes evident when one considers that absolute smoothness at the points of contact between the body and the supporting horizontal surface is necessary.

If this standard body is duplicated as nearly as possible, and the two standard bodies are caused to interact by tieing them end to end and pulling them apart, then the extensions of the two bodies will be equal. This leads to the principle, which is equivalent to Newton's third law, that *when one body exerts a force on a second body, the second body exerts an equal and opposite force on the first.* This may be taken to be a generalization of a simple experience to include all cases of force interaction in all directions and for all conditions of motion. If we were to accept this principle beforehand it would be unnecessary to duplicate the original standard body in all respects, because the distortion of any other body under unit force could be found by interaction of the other body with the single standard body.

It seems necessary at this point to introduce another principle which is equivalent to Newton's first law, to the effect that *the summation of the forces on a body in every direction is zero if the body has no acceleration.* This might be called the principle of equality of forces. It permits us to use intermediate bodies for transmitting known forces. Two standard bodies may be yoked together in parallel and the yoke tied to a body to which two units of force are to be applied. If the yoke is not accelerating, then two units of force must be applied at each end of the yoke in opposite directions, and by Newton's third law the two units of force from the standard bodies are transmitted to the body which is to be acted upon. Also, by means of intermediate yokes, the pull of standard bodies can be translated into a push on other bodies.

In order to measure vertical forces, in the absence of standard bodies of finite rigidity and zero mass, it is necessary to be quite arbitrary in definition. We shall say that *the tension in a massless string extending over a perfect pulley is the same at both ends.* To use this axiom requires extrapolation to zero mass of the string (a simpler problem than extrapolation to zero mass of our standard body) and to the perfect pulley. The perfect pulley has zero diameter at its bearing journals and it must be balanced. That is, it must rotate slowly upon infinitesimal disturbance, but it must never rotate spontaneously whatever the position of the wheel.

The justification of this axiom for forces in the horizontal plane only can be readily made by measuring the string tension at both ends. A partial justification for forces in vertical and horizontal planes can be made by using vertical measuring bodies of various mass-rigidity ratios. As the mass approaches zero for a given rigidity, the deflections of duplicate bodies at the two ends of the string will approach identity. Nevertheless, the axiom concerning equality of tension must remain funda-

mentally a rule for measuring vertical forces and, as such, a part of the definition of force.†

From our definition of force and the principle of equality of forces, it follows that all bodies are acted upon by a vertically downward force called gravity. This is evident from the fact that in order to prevent acceleration of any body it is necessary to provide a vertically upward force on it. This force—which may be measured—is, by the principle of equality of forces, equal in magnitude and opposite in direction to the force of gravity. The gravity force is obviously not the result of interaction by contact between the body in question and some other body, as the measured supporting force is. Gravity is thus an action at a distance. Such forces are usually measured indirectly by providing a counteracting measurable contact force which results in zero acceleration.

Thus far we have defined stationary force; that is, force which is applied to a body which is not accelerating. This definition appears to be necessary to an exposition of the science of statics; but in this field, as in dynamics, the early investigators did not adequately define their terms. Some of the concepts expressly stated in the definition given above were implied in early discussions of statics. For example, the principle of equality of tension in a string stretched over a perfect pulley was implied in the work of Stevinus approximately a century before the enunciation of Newton's laws. Force was apparently measured by weights—that is, by the number of units of mass that would be supported against gravity. Newton was aware that two forces that appeared to be equal by this method might in fact have very different effects if one were measured on a high mountain and the other at sea level. The deformable body, as a force standard, is free from this defect.

Poincaré objected to the dynamometer, or the elastically distorted body, as a means of measuring force because it would be affected by "temperature and a multitude of circumstances." The effect of temperature can be readily controlled. The effect of orientation can be avoided, as indicated above. The effects of the other multitude of circumstances seem to be of less consequence. For example, the pressure of the surrounding atmosphere may be controlled, although its effect within terrestrial variations is quite negligible. It seems probable that the remainder of the multitude of circumstances referred to by Poincaré would equally affect the prototype of the unit of mass which seems to be entirely acceptable.

†The equal-arm balance for measuring mass is, in a sense, a special case of the perfect pulley.

In dynamics forces must be applied to bodies that are accelerating as well as to those that are stationary. Since a force measured by distortion of a standard body which is accelerating may not (and indeed will not) have the same effects as one measured by an equal distortion of the standard body when stationary, it is necessary to devise a means of measuring moving forces in terms of stationary standard bodies.

Suppose that a string which applies a force to a moving body extends over a perfect pulley so that the two ends of the string are at right angles to each other. If the pulley bearings are held fixed by means of two stationary forces in the directions of the two ends of the string, then each stationary restraining force is equal to its opposite moving force. This procedure seems to be justifiable by the principle of equality of forces and Newton's third law in combination. If it were not so, then this procedure would become part of the definition of a moving force.

We are now prepared for the statement of Newton's second law in terms of the measurable quantities length, time, mass, and force. It may be stated as follows: *An elementary body is accelerated in the direction of the resultant force on the body, and the product of the mass of the body and its acceleration is proportional to the resultant force.* The resultant force is the vector sum of all forces. An immediate corollary is that the effect of each force acting on a body is independent of the other forces acting on the same body—that is, that a force in the X direction results in the same acceleration in the X direction whether or not a force acts in the Y direction.

It appears that the definition of force is not simple and not easily devised. Perhaps the one who rashly treads this ground exhibits something less than the wisdom of angels. On the other hand, the definition of heat—which proceeds through equality of temperature and temperature change in arbitrarily restrained standard bodies—is neither simple nor easy. Nevertheless, this definition has illuminated the science of thermodynamics.

It seems probable that the science of dynamics will be illuminated by a similar treatment, a similar insistence on the rules for counting to get the numbers which go into the equations. These procedures for counting are what distinguish the science of dynamics from the science of mathematics. It may safely be said that this distinction has not been evident to many students of dynamics.

The proposed procedure also emphasizes the arbitrary nature of the quantities in terms of which the science is expressed. It disperses much of the

hoary atmosphere of the metaphysical that still clings to Newtonian mechanics, despite the limitations emphasized by the theory of relativity. It makes it seem as ridiculous, for instance, to try to explain thermodynamics in terms of dynamics as it would be to explain dynamics in terms of thermodynamics.

Ernest L. Robinson once said that in his youth he felt that Newtonian mechanics was superfluous. He did not need to be told by Newton why an apple fell to the ground—it fell because the stem rotted. Newton's point of view was no more logical than Robinson's. It was justified only because it clarified relationships between phenomena in a way that Robinson's apparently did not.

And when the student has come to understand this heuristic quality of science, he can enter a new science without violent readjustment. Undergraduates who have studied the methods of counting peculiar to dynamics will doubtless find thermodynamics less strange when they come to it a year or so later. It is entirely possible that the theory of relativity will be to them just another quantitative science of no unusual difficulty.

Mechanics which is simply more mathematics—more equations, more calculus—is not true mechanics. Mechanics which requires the student to know the meaning of symbols, the significance of the relationships between them, the distinction between definition and principle, is an exercise in logic and inquiry. It is an invitation to the student to examine critically and freely, to probe deeply and skeptically, the subject he studies. Thus even mechanics can be made to contribute to the development of what may be the last great hope of mankind, the free spirit of inquiry.

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THE GREAT EVENT

(ACCORDING TO WAVE MECHANICS)

Arrayed in dark and bounded space, the diamond-studded horseshoe tensely beams.
Drums roll. The baton of the Great Conductor, plummeting from high,
Sounds the mightiest of chords in an endless symphony—
A clash of symbols and out of cold and fury flashes a spinning fiery jewel,
A vibrato in the harmony of singing strings.

Now a hush—the woodwinds chant its lustrous beauty.
Then a rumbling bass—a string is plucked, another, then another
As her radiant arms drop dancing pearls in Kepler's orbs,
Happy silent images sparkling in the brilliant light,
A ballet of ruby, emerald, and gold.

G. W. GRIMM

THE OUTLET OF LAKE BONNEVILLE*

RONALD L. IVES

Mr. Ives (M.S., University of Colorado) has long been interested in the geography of the West, about which he has published a number of articles. Now in the Department of Geography, Indiana University, he found time, after his war service, and before going to this post, to do extensive work in the Lake Bonneville region.

THE Pleistocene lakes of North America constitute one of the most fascinating and most challenging fields for research in modern geology, despite the extensive and competent pioneer work by such men as G. K. Gilbert and I. C. Russell. Perhaps the most familiar of these ancient bodies of water is Lake Bonneville, which once covered parts of Utah, Nevada, and Idaho to a maximum depth of more than 1,000 feet.

So extensive and obvious was the evidence for this lake that the first literate white man to enter the area, Fray Silvestre Velez de Escalante, wrote in his journal, on October 2, 1776, ". . . This place, which we named Llano Salado, because we found some thin white shells there, seems to have once had a much larger lake than the present one."[1, 2] These "thin white shells," which may have been *Lymnaea bonnevillensis,* later helped Gilbert and others to reconstruct climatic conditions at the time of high lake levels.

About three quarters of a century later, Captain Howard Stansbury, in the course of his official duties, made a survey of the valley of Great Salt Lake, noting, during the progress of his work, the presence of a number of "benches" on the mountains. From these, which he correctly identified as ancient shore lines (Fig. 1), Stansbury arrived at a reasonable estimate of the dimensions of the former lake, description of which is given in his official report.[3]

Shortly thereafter, there came to the Great Basin region, as a member of an exploring party, a young geologist named Grove Karl Gilbert, a keen observer who devoted an appreciable part of his highly productive working life to the study of this ancestor of Great Salt Lake. His report, "Lake Bonneville,"[4] is not only a classic of geological exposition, but still constitutes the "bible" of all geologists working in the Great Basin region.

From the publication of Gilbert's report in 1890 until about 20 years ago, further progress in the study of Lake Bonneville awaited significant "advances in the art" of geology. Then studies of Pleistocene climates, by Blackwelder,[5] Antevs,[6] and others, showed that climatic changes like those which produced Lake Bonneville also took place in other parts of North America, and that perhaps a stage-for-stage correlation of Pleistocene lake levels with Pleistocene glacial advances could be made. Finding of evidence of ancient man in some sea caves along the abandoned shores of Lake Bonneville, by M. R. Harrington, of the Southwest Museum, and others, added impetus to this study (Fig. 2). Although much progress has been made in these correlations, their present status calls for continued application of the concept of multiple working hypotheses.

History and extent of Lake Bonneville, as reconstructed by Gilbert, are competently outlined in his monograph, with its accompanying maps (Map References), and are shown, in summary form on a modern base, in Figure 4. Modern work confirms all Gilbert's factual data, and most of his conclusions, but has so augmented the evidence that the following history now seems to merit consideration:

1. During middle or early Pleistocene the Great Basin was occupied by a series of playa lakes, their number, their extent, and their interrelations, if any, being still undetermined.

* During the course of this investigation, valuable assistance, ranging from trail information to help in "unsticking" jeeps, was received from many residents of isolated mining camps and ranches in the Bonneville area. This is here gratefully acknowledged, as is the hospitality so generously offered, during wartime food shortages, at many Utah, Idaho, and Nevada ranches.

Profiles of many parts of the area were furnished at various times by the U. S. Geological Survey; the U. S. Bureau of Public Roads; the highway departments of the states of Nevada, Utah, and Idaho; and the Union Pacific Railroad. Access to files of aerial photographs, made possible by the U. S. Forest Service, the U. S. Department of Agriculture, and the U. S. Army, saved many miles of travel and permitted double checking of most aerial observations.

The writer is indebted to Col. John R. Burns, CWS, for assistance during parts of this study; to the late George Langham, Capt. A.C., for skilled pilotage during aerial reconnaissances; to Maj. R. A. Davis, CWS, for check observations and for the photographs comprising Figures 5 and 6; and to Dr. Wallace T. Buckley, of Indiana University, for helpful suggestions during the preparation of this paper.

Fig. 1. Aerial view of Reservoir Butte, a former island in Lake Bonneville, shows Bonneville (*upper*) and Provo (*lower*) shore lines. Between Reservoir Butte (*center*) and the Simpson Mountains (*on horizon*) is the ancient river bed joining the Sevier Desert and the Salt Lake Desert. Distance between the two shore lines is about 350 feet.

2. Immediately thereafter the region became at least as dry as at present, and so remained for a period measurable in tens of thousands of years. During this time massive fans were constructed at the bases of the mountains.
3. Early in the latter half of the Pleistocene, water rose in the Bonneville Basin to within about 90 feet of its maximum level—the Bonneville shore line. This level was retained continuously for a period long enough to permit the deposition of up to 200 feet of laminated yellow marl.
4. Water level fell to an undetermined low level and remained there for an undetermined period, probably much shorter than (2).
5. Water again accumulated in the basin, rising to the Bonneville shore line, and remaining there long enough to permit the deposition of up to 50 feet of thin-bedded white marl. This marl, and the yellow marl previously deposited, effectively seal the earlier fan deposits at their lower ends, creating an extensive buried aquifer, from which irrigation water, of some present and considerable future economic importance, is pumped.
6. The lake level declined from the Bonneville shore line to an undetermined low level, and for an undetermined time, probably short.
7. The lake again rose, attaining a height slightly above the Bonneville shore line, then declined fairly rapidly, with a series of minor stadia, to the Provo shore line, leaving the area between the two shore lines terraced with abandoned beaches (Fig. 3). This decline in level was, correctly, attributed by Gilbert to the lowering of the outlet from the basin at Red Rock Pass (Fig. 4, pass index, 1).
8. After a long pause at the Provo shore line, lake levels again declined, in a series of short-lived stadia (Fig. 3), to the Stansbury shore line, which may, in part, have been "left over" from a previous low-water regime; and then declined to or below the present level of Great Salt Lake.

Although this history is at present somewhat complex, a not-inconsiderable volume of evidence suggests that there may have been additional stages, Antevs, for example, postulating a dual occupation of both the Provo and Stansbury levels.

THE PROBLEM OF OUTLETS

Early in the course of his study of Lake Bonneville, Gilbert concluded, with excellent logic, that long-time stability of lake levels was only possible if the lake had a definite outlet and, hence, that Lake Bonneville must have had an outlet, for deeply incised shore lines, such as the Bonneville and Provo, could only be produced by long-continued wave action at a constant level. From the field evidence then available, Gilbert determined that this outlet had an elevation approximating that of the Bonneville shore line during earlier stages in lake history, and that then was lowered to the Provo level, probably by erosion, during a later period.

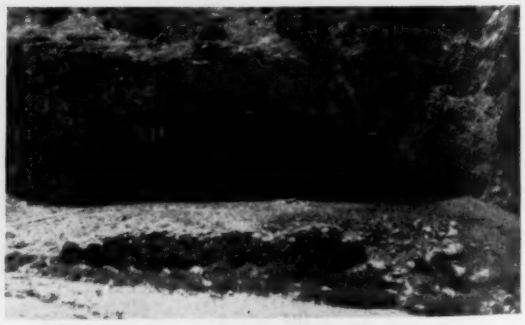

Fig. 2. Sea cave on abandoned shore of Lake Bonneville, near Magna, Utah. In some of these caves, evidence of ancient man has been found.

Systematic investigation of the periphery of the Bonneville Basin, by Gilbert and his aides, disclosed a number of low passes in the basin rim. Further exploration showed that Red Rock Pass (Fig. 4, pass index, 1) not only was the lowest pass, but showed incontrovertible evidence of former great outflow from the Bonneville Basin to the Snake River. This discovery, reported in 1878,[7] solved the outlet problem as it was then understood, and today explains in satisfactory detail the decline in lake level from the Bonneville shore line to the Provo shore line, and the relatively long-term stadium at the Provo level.

Not explained by Gilbert's findings are the stabilization of lake levels at and near the Bonneville shore line during the deposition of the white marl and of the older yellow marl; and the factors permitting low-level stabilization (perhaps several times) at the Stansbury shore line. Although the possibility of other and older outlets was considered by Gilbert and others,[8] and very extensive surveys have been made in Utah since the termination of Gilbert's field work (1885), the problem remains unsolved to the present time, and at least one more outlet, to account for the early stabilization of lake levels, is called for by most current theories.

During the period 1928–1946, the entire problem of the outlet of Lake Bonneville was reviewed in considerable detail, in conjunction with attempts at correlation of glacial and pluvial stages from the California Sierras to the Colorado Rockies. This investigation, after preliminary cartographic study (Fig. 4; Maps), included an aerial reconnaissance of the entire periphery of the Bonneville Basin, and ground studies in all passes (Fig. 4, pass index) leading from the basin to exterior drainages.

Lower limit of the area in which outlets of Lake Bonneville might be found is quite obviously the Bonneville shore line, there being neither evidence nor suspicion of underground outlets at present or in the past. Consideration of post-Bonneville deformation of the region[9] indicated that, in some areas at least, such as the Escalante Desert in southwestern Utah, a pass now as much as 5,450 feet above mean sea level might have served as an outlet in Bonneville time; and backward extrapolation of this deformation suggested that all passes below the present 6,000-foot contour should be considered. In consequence, the search area (Fig. 4) was defined as all parts of the Great Basin below the 6,000-foot contour, from the Bonneville shore line to the 5,000-foot contour of an exterior drainage. During the course of field checking of these areas, a large part of Gilbert's field evidence was inspected and verified.

RED ROCK PASS

Occupying a structural valley between the Bannock and Portneuf Ranges, about 90 miles north of Odgen, Utah, is Red Rock Pass (Fig. 4, pass index, 1), the lowest gap in the periphery of the Bonneville Basin. This pass is today traversed by the main railroad from Ogden, Utah, to Pocatello, Idaho, and by a paved highway connecting the same points. Highest point in the pass is about 4,380 feet above mean sea level, an elevation far below the Bonneville shore line, and only a few feet above the Provo.

During his investigation of Lake Bonneville, Gilbert visited Red Rock Pass, studied it in considerable detail, and published a clear, concise description (1878), which was later expanded and augmented by maps and sketches.[10] Gilbert found that the Bonneville shore line entered the southern part of the pass, but terminated abruptly near its center, at the gap between a pair of red limestone buttes on opposite sides of the valley (Figs. 5, 6). Floor of the pass was level, and very slightly above the Provo shore line, but contained evidence of recent (post-Provo) filling. North of the Red Rock buttes (Fig. 6), the valley floor slopes gently northward, draining into the Snake River via Marsh Creek and the Portneuf River.

Near the present town of Arimo, which is about 18 miles north of Red Rock Pass, the valley of Marsh Creek contains a lava tongue, of pre-Bonneville (probably later Tertiary) age, on each side

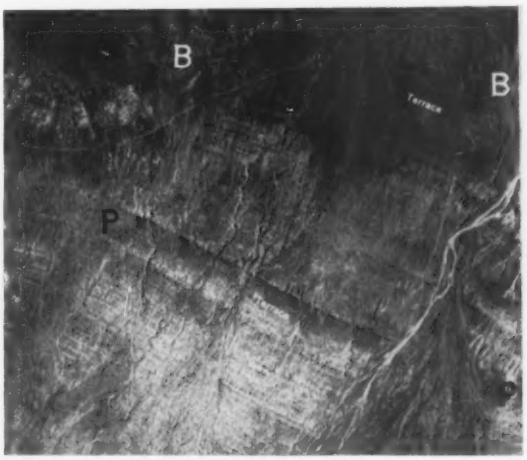

Fig. 3. Major and minor shore lines of Lake Bonneville, as seen from a terrain clearance of 20,000 feet. *B*, Bonneville shore line; *P*, Provo. Other terraces, parallelling these, were cut and constructed during minor lake stadia.

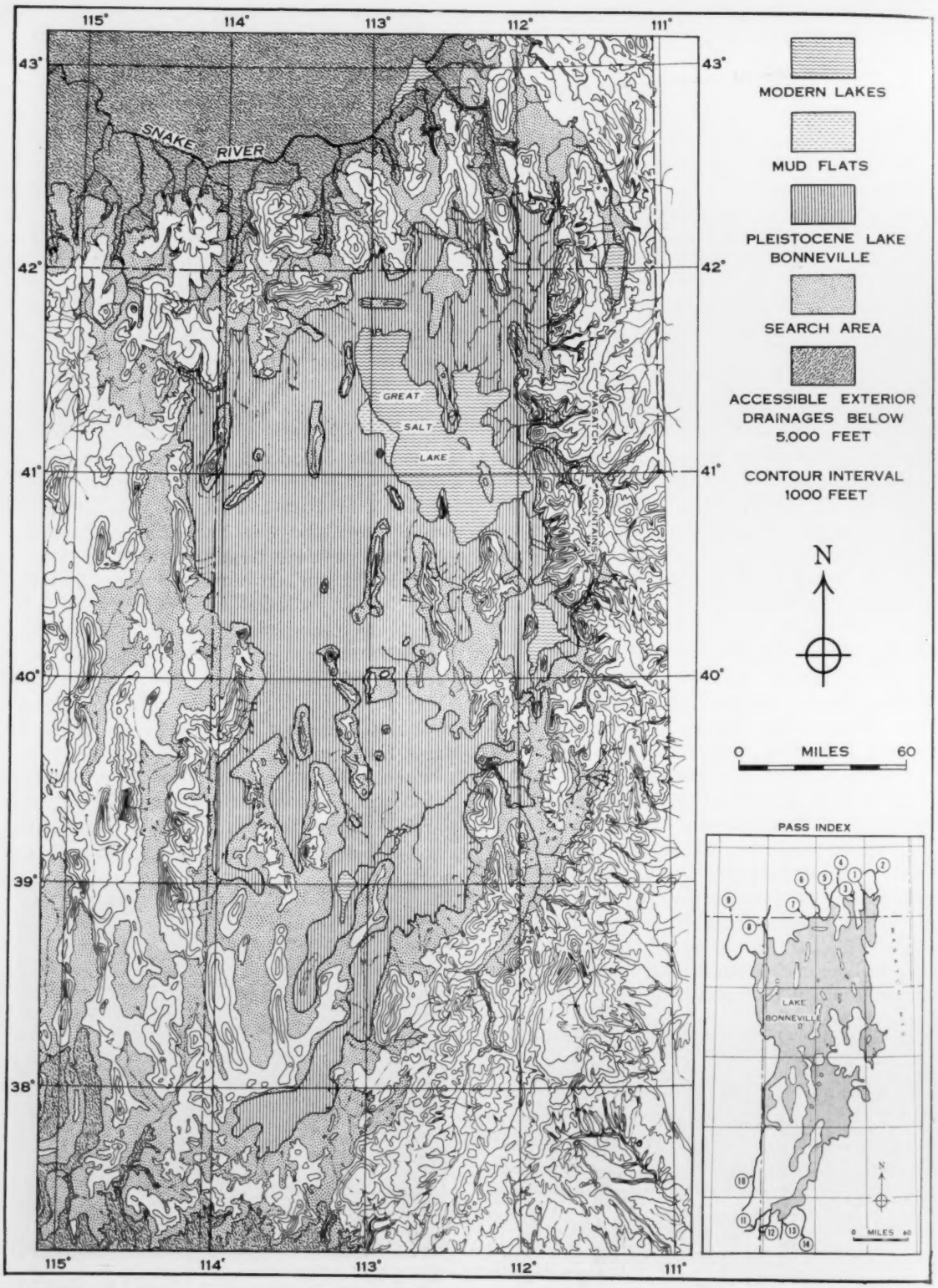

Fig. 4. Summary map of the Bonneville Basin, showing modern lakes and mud flats, extent of Pleistocene Lake Bonneville, "search area" in which outlets should be, and accessible exterior drainages. "Pass index" in lower right is for identification purposes.

Fig. 5. View south in Red Rock Pass, from the east wall. Note steep inner bank of channel eroded in alluvium on west wall below Oxford Mountain (*upper right*). Swamp in middle distance is the result of post-Provo interruption of drainage, due in part to local deposition.

Fig. 6. Red Rock Pass from the north, showing the red limestone buttes (*middle distance, right and left*), the flat-floored channel (*center*), and the alluvial fans (*right and left*) which formerly coalesced, damming the channel to a level above the present 5,200-foot contour.

of which a deep channel has been eroded. Today, the eastern channel is occupied by the Portneuf River and some of its tributaries, and the western channel, parallel and only a few hundred feet distant, carries the waters of Marsh Creek. Several to many yards above the present stream levels, the upper surface of the lava is carved, fretted, and potholed by the now-vanished torrent that once flowed through Red Rock Pass from the Bonneville Basin.

Gilbert concluded from this evidence that Red Rock Pass was the overflow point in the rim of the Bonneville Basin during the last rise of the water to the Bonneville shore line, that the fall to the Provo level was caused by the washing out of the barrier of alluvial material formerly blocking the pass, and that the stabilization at the Provo level occurred when erosion reached bedrock in the bottom of the pass.

Thorough field checking of this area indicates that Gilbert's findings are entirely correct, a conclusion also reached independently by Major R. A. Davis, CWS. Valley wall deposits north of Red Rock Pass were scrutinized with considerable care because of Peale's contention (1878) that the outlet of Lake Bonneville was near the Marsh Creek–Portneuf junction (Map S), rather than at Red Rock Pass. A few vague indications of shore lines were noted from the air between Red Rock Pass and Downey, and a number of small isolated masses of alluvium, similar to terrace material, were found at various elevations above the valley floor in different parts of this region, but no evidence was found to relate them either to each other or to any phase in the history of the Bonneville River. Some of this material appears older than Pleistocene.

The possibility that the dam of alluvial material in Red Rock Pass was washed out during an early lake stage, after stabilizing the level for a considerable time, and then was rebuilt by further deposition, to repeat the process at a later stage, producing the evidence now present and obliterating the evidence of earlier outflow, was suggested by Gilbert.[11]

Because this suggestion is not only in rational accord with standard geological mechanics, but would also account for some or all of the "troublesome" early stabilizations of lake level, a rather careful investigation of deposition rates in this area was made. Although present datings of Pleistocene occurrences still leave much to be desired, we can, for the purposes of this study, assume (following Antevs) that the last outflow through Red Rock Pass took place not less than 20,000 years ago. Depth to bedrock, in the approximate center of Red Rock Pass, as shown by excavations for the highway overpass (Fig. 5, center), is about 15 feet, half the fill being bog deposit mixed with alluvium, the lower half being hard (stream gravel). If it is assumed that all this material is post-outflow deposition, an average rate of deposition of about 0.00075 foot, or about 0.01 inch, per year results. Because the rate of radial growth of an alluvial cone declines as the radius and height increase, a linear extrapolation, giving a time of growth of 350,000 years for the barrier, is incorrect and excessive. When due corrections are made, it appears that the time necessary for the filling of Red Rock Pass from the Provo to the Bonneville level (about 350 feet) is not less than 250,000 years.† As this time interval is almost twice the probable total time since the earliest high level in the Bonneville Basin (the yellow marl, or "Escalante," stage), as is indicated by Flint's latest age estimate,[12] it appears that Gilbert's plausible suggestion cannot be applied in this specific case because of inadequate time.

In consequence, at least one outflow from the Bonneville Basin, prior to the final break-through at Red Rock Pass, is called for by present theories and must have occurred at some location other than Red Rock Pass.

THE GENTILE VALLEY–BEAR RIVER–PORTNEUF RIVER AREA

Easternmost of the topographic "lows" connecting the Bonneville Basin with the Snake River drainage consists of a tortuous series of valleys and lava flows between the Bear River (Map A) and the Portneuf River. In this area, a long arm of Lake Bonneville once extended up Gentile Valley toward Grace, Idaho, where the delta of the Bear River was located during high-water stages.

From the northernmost point on the 5,100-foot contour in Gentile Valley (a level approximately that of the Bonneville shore line) to an equal elevation in the valley of the Portneuf is less than 15 miles, with no intervening elevation exceeding 5,800 feet. Land surface in many parts of this area, locally known as Basalt Valley, is composed of basalt flows, some definitely of Tertiary age, others

† Simplified formula for cone growth is:

$$\frac{dr}{dt} = C\left(\frac{Z - r\tan\theta}{r^2\tan\theta}\right),$$

in which:

$\frac{dr}{dt}$ = rate of increase of radius of cone
C = constant of proportionality
Z = height of source cliff
r = radius of cone
θ = angle of rest of component materials

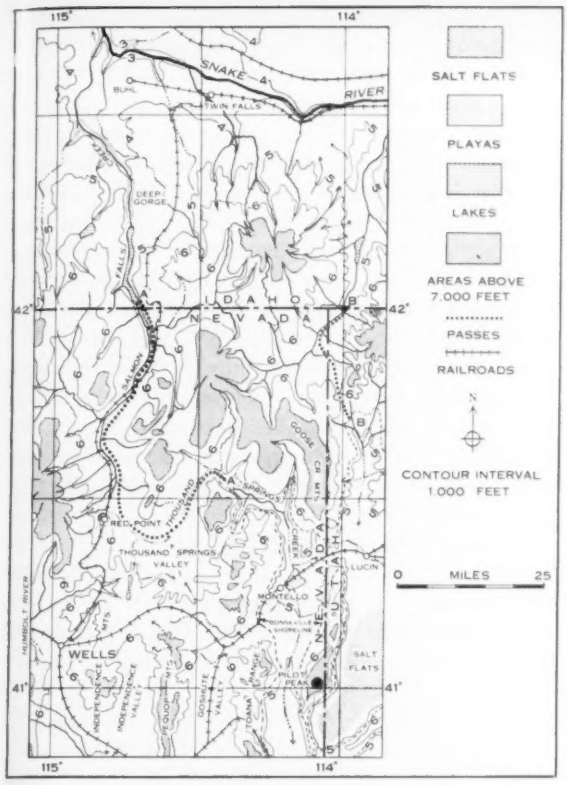

Fig. 7. Summary map of the Thousand Springs area, showing axis of pass (*A*—*A'*), major stream channels, and elevations. Contour designations are in thousands of feet. Secondary pass (*B*—*B'*) is Goose Creek Pass (No. 8 in Fig. 4).

younger, possibly Pleistocene. Beneath this basalt, which has a thickness exceeding 250 feet in many places, is alluvium, of probable Tertiary age, locally interstratified with lavas, and carrying large quantities of water, probably from the Bear to the Portneuf River. This suggests that an ancient channel of the Bear River, which once connected with the Portneuf, has been obliterated by lava flows, which may explain the "looped-back" course of the Bear River. (Such looped courses are quite common throughout the Basin-and-Range Physiographic Province, other prominent examples being the Sevier River in Utah and the Rio de Bavispe, in Sonora.)

Aerial reconnaissance of the lava region disclosed no clear evidence that a channel through the older lavas (Tertiary) had been filled by later flows (Pleistocene, with much doubt). Ground investigation confirmed this finding, and showed that there was no close agreement between the positions of present topographic "lows" and the positions of later lavas. Backward extrapolation of regional distortions gave no indication that these lavas were depressed far enough, at any time in the Pleistocene, to permit outflow across them, either from Bear River or from a lake in the Bonneville Basin.

Investigation of the valley of the Portneuf, from Soda Springs (Map A) to McCammon, showed plainly that it has not carried, since the present channel was eroded (Tertiary), any flow greatly in excess of that at present. Shifting of the channel of the Portneuf, from McCammon northward, from the west side of the Marsh Creek lava tongue (eroded by outflow from Red Rock Pass) to the east side, after the lowering of Lake Bonneville, is indicated by the presence of a partly obliterated channel across the lava approximately 11 miles south (upstream) of the present Portneuf–Marsh Creek junction. Detailed descriptions of this area are contained in the journals of Captain Bonneville, who wintered at least once a few miles east of this junction, near "Beer Springs," on the Portneuf.

Assuming the substantial correctness of Mansfield's datings[13] of the lavas in Basalt Valley, and the absence of evidence of any marked change in relative levels, it appears that the Gentile Valley–Bear River–Portneuf River area was not an outlet from the Bonneville Basin during the later part of the Pleistocene.

THE NORTH RIM PASSES

West of Red Rock Pass, on the north rim of the Bonneville Basin, there are six passes (some having multiple adjacent channels) below 6,000 feet from the basin to the Snake River drainage (Fig. 4, pass index, 3, 4, 5, 6, 7, 8). This drainage divide is locally known as the Raft River Mountains. This entire area was studied in some detail by Gilbert, who found that the Bonneville shore line was below the summit of each pass. Restudy of this area, using modern maps and allowing for regional distortion at a uniform rate, indicates that none of these passes was in position to serve as an outlet during the last half million years.

All these passes are easily accessible by road, and all have been surveyed in some detail. Ground study indicates that not only is there no direct evidence of flow over the pass summits, but that the channels extending from the passes toward the Snake River show no evidence of formerly augmented through flow. Thus, although the Raft River area is of considerable interest to the modern physiographer and geomorphologist, it apparently does not offer a solution to the problem of outlets during the Pleistocene.

THOUSAND SPRINGS PASS

At the northwest corner of the area under consideration is Thousand Springs Pass, an important topographic low supplying a workable railroad route from northeastern Nevada to the Snake River Valley, as well as a low saddle from the Bonneville Basin to the headwaters of the Humboldt River, an important transcontinental thoroughfare since 1849. Despite its importance, parts of the area have never been mapped in detail, and most modern maps are based on Beckwith's reconnaissances, made in 1861 (Map T). A map of this area, based on extant maps, aerial photographs, and modern ground studies, comprises Figure 7.

The Thousand Springs area has had a most complex geological history, only a small part of which pertains to the problem at hand. The lower part of the pass, in the vicinity of Montello, is now a small playa, although, during high stages in the Bonneville Basin, it was a part of the main lake. A small bar separates the western part of the basin from the main Salt Lake Desert, so that there is now no "through" drainage. This may be in part the result of relatively recent uplift of a buried northward extension of the Pilot Range. Because the Bonneville shore line is locally obscured in many places by slope wash, local evidence of distortion is somewhat inconclusive.

Northwest of Montello, draining Thousand Springs Valley, is the canyon of Thousand Springs Creek, a rugged canyon incised at the junction of the Toana Range, composed largely of Paleozoic sediments and metasediments, and the Goose Creek Mountains, a mass of Tertiary lavas, with occasional inclusions of granite and black limestone.‡ Several faults roughly parallel the canyon, and parts of its course, seem to be eroded through zones of brecciation.

The Bonneville shore line can be traced into the canyon, but not through it. Toward the western end the shore features merge with, and are covered by, stream terraces related to high stages of Thousand Springs Creek, an ephemeral torrent. In many places both stream and lake deposits are concealed (or perhaps removed) by local "cloudburst" deposits. Some maps show an "improved" highway through this canyon. The present road (1947) can be traversed without too much difficulty by a jeep or half-track with the "creeper" mechanism in good condition.

‡ This material closely resembles the granite and black limestone at Granite Peak (Lat. 40° 07′ N.; Long. 113° 15′ W.: Map B: Ives, R. L. The Granite Peak Area. *Utah, Rocks and Minerals*, 1946, **21**, 339–46.

Thousand Springs Valley (Fig. 7) consists of a series of basins, which are interconnected during stages of high water but are independent "blind" playas during ordinary seasons. During high-water stages at present (and during Pleistocene pluvial stages), these playas overflow through Thousand Springs Canyon into the playa at Montello. The oldest sequence of playa deposits, now somewhat tilted, is quite ancient, and may be a part of the Humboldt Formation, of Pliocene (?) age. A second sequence, only slightly tilted, is placed in the later Pleistocene and probably represents the same pluvial climate that produced the high stages in the Bonneville Basin. The most recent deposits, still forming, are quite thin compared to the others, and are currently undergoing cut-and-fill erosion and redeposition, suggesting a recent change in local erosional equilibrium.

North of Thousand Springs Valley, and separated from it by a pass approximately 5,850 feet above m.s.l., is the canyon of Salmon Falls Creek, a tributary of the Snake River. This canyon, very deeply intrenched in some places (Fig. 7), is fairly

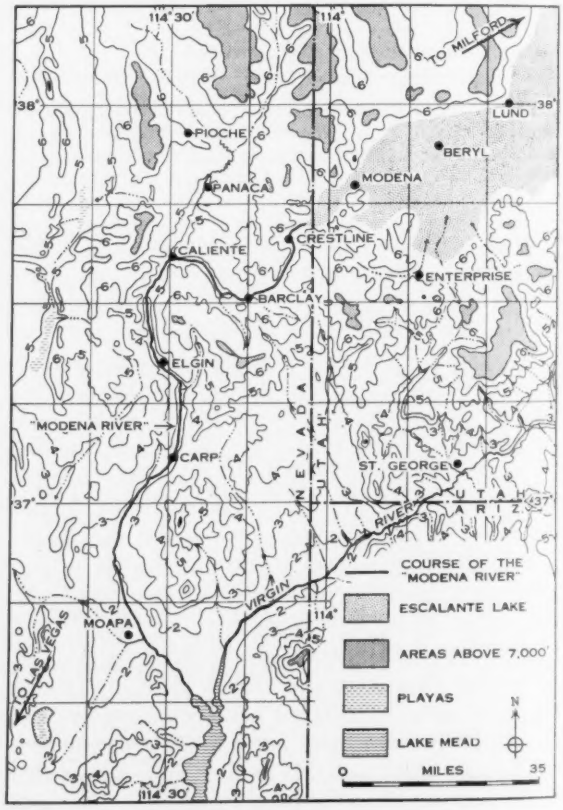

Fig. 8. Sketch map showing course of hypothetical Modena River from the Bonneville Basin to the present course of the Colorado River. Contour lines, designated in thousands of feet, are modern.

typical of the Snake River lava area and of its rainfall regime, and bears no convincing evidence of having carried, in the Pleistocene, any increase in flow not compatible with local rainfall increases at that time.

For Thousand Springs Pass to have served as an outlet for a lake in the Bonneville Basin at the Bonneville level (*ca.* 5,100 feet above m.s.l.), the summit of the pass, at that time, must have been depressed to that level, or about 750 feet relative to its present position. Measurements of the deformation of the Bonneville shore line indicate a maximum elevation of only about 15 feet at the west end of Thousand Springs Canyon since the Bonneville high-water stage. The most generous backward extrapolation of this warpage rate suggests that the summit of Thousand Springs Pass was about 700 feet too high, during the time under consideration, to serve as an outlet from the Bonneville Basin. Similar considerations suggest that there was no connection between the Bonneville Basin and the Lahontan Basin, to the west, during the later Pleistocene; but field study suggests a considerable "swapping of tributaries" by stream piracy during this time. Unless new evidence is found, it appears that Thousand Springs Pass is another of the places where Lake Bonneville and its predecessors did not overflow.

THE WESTERN PASSES

Along the western margin of the Bonneville Basin, above the Bonneville shore line and below the 6,000-foot contour, is a maze of interconnected and poorly drained desert valleys having a total length of perhaps 600 miles (Fig. 4). These, typical bolsons, contain a large number of ephemeral playa lakes and resemble, in their major features, the Jornada del Muerto area of New Mexico.

Shortest channel from Lake Bonneville to the Colorado River (Fig. 4, pass index, 10) is through Snake Valley, in which there are two high points, both more than 5,900 feet above mean sea level. Present (and ancient) drainage from the area between these barriers is outward, one flow going into the Bonneville Basin and the other, via Duck Valley, going to the Colorado.

Thorough aerial and ground study in this area shows that all flow of which there is any remaining evidence was out of the maze of valleys, not through it; and that, when regional tilting is considered, even most generously, the area could not have served as an outlet during the time under consideration.

CRESTLINE PASS

Joining the southwestern extension of the Escalante Desert with the valley of Meadow Creek is Crestline Pass (Fig. 4, pass index, 11), a depression between the Mormon Mountains (southeast) and the Clover Valley Mountains (northwest). The western end of this pass drains into Meadow Creek via Clover Valley, and eventually to the Colorado River. The old California and Salt Lake Stage Road traversed this gap, which today is used by the Los Angeles and Salt Lake Railroad.

Aerial and ground reconnaissances of the Crestline Pass area disclosed that the eastern side of the pass, extending from Crestline (Nevada) to the Escalante Desert (Utah), was a typical low-gradient valley, like several dozen others on the north,

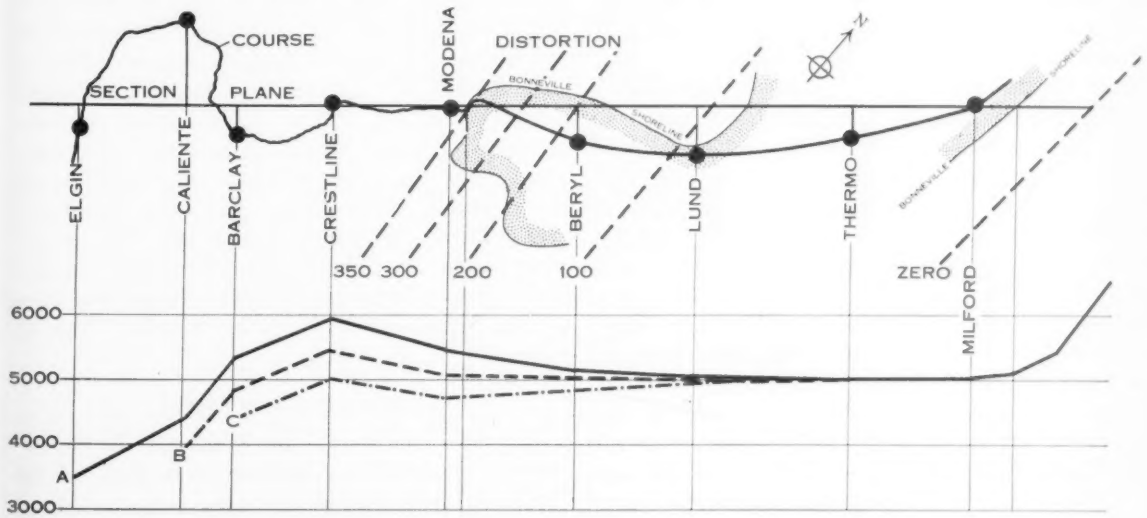

Fig. 9. Present and reconstructed profiles across a section from Elgin, Nevada, to Milford, Utah.

west, and south sides of the Bonneville Basin, with some erosion at its head and some fan deposition where the pass joined the main Escalante Desert.

The western side of the pass, however, from Crestline to Caliente, is disproportionately deep for its drainage area and has the general appearance, from any aspect, of the abandoned channel of a fairly large desert river. This valley is clearly visible on the north side of the main airway from Salt Lake City to Las Vegas, Nevada.

Ground investigations disclosed a considerable depth of fan materials in the valley and indicated that climatic and depositional conditions have been substantially like those of today for a period sufficient to permit accumulations exceeding 1,000 feet thick in some places. Whether this entire sequence is all Pleistocene, or whether it includes both Pleistocene and later Tertiary deposits, including perhaps the Panaca formation,[14] was not satisfactorily determined from either field evidence or stratigraphic descriptions.

Plentiful evidence of cut-and-fill action, normal to valleys having violently fluctuating flow rates, was found in many parts of the valley, at various levels. No convincing evidence of long-continued recent steady flow conditions was found anywhere in the area.

Because the terrain west of Crestline strongly suggests deep and geologically recent trenching of an area having little rainfall, and probable recent connection of formerly discrete playa drainages into the system now flowing into the Colorado River through Meadow Valley and Muddy River, the former existence of a fairly large river, here called the Modena River, can be postulated. Topography of the course of the hypothetical Modena River is shown in Figure 8. It will be noted that the channel here is more deeply incised than that of the Virgin River in some parts of the course.

Regional warping since the cutting of the Bonneville shore line was measured by Gilbert. These data have been transferred to Figure 9, and from them a series of profiles has been constructed. The first of these (*A*, Fig. 9) is the modern profile from Elgin, Nevada, to Milford, Utah. The second (*B*, Fig. 9) is a profile of the same area during the Bonneville Lake stage, it being assumed that Gilbert's measurements of distortion (Fig. 9, "distortion") are at least substantially correct. From Gilbert's researches, the time interval between the deposition of the yellow marl ("Escalante Lake Stage") and the cutting of the Bonneville shore line is shown to be somewhat greater than the total time lapse since the cutting of the shore line. If these times are assumed equal, and distortion for the whole period proceeded at a substantially uniform rate, the profile during the Escalante Stage approximates that shown in *C* (Fig. 9). Note here that the low point of the basin has shifted from near Modena in Escalante time to near Thermo at present. This shift may account for the apparent "pinch-off" of the lake near Thermo,[§] the apparent duality of the Bonneville shore line in the southern part of the basin, and the lack of register of successive deposits in this area.

Consideration of profile *C* shows that the waters of the Escalante Stage, which rose to within 90 feet of the Bonneville shore line at Leamington, on the axis of zero distortion, could have overflowed through Crestline Pass; and hence that such an outlet, through the Modena River, is a possibility!

Using the same basic data and various other assumed positions of the "hinge point" (chosen just west of Milford for Fig. 9), and also various distortions other than a simple tilting of the block extending from Elgin, Nevada, to Milford, Utah, additional profiles (not shown), of various curvatures and magnitudes compatible with Gilbert's distortion figures, were constructed. All these produce a topographic "low" at or near Crestline, during the Escalante Lake Stage, at a level sufficiently low to permit outflow through it. In fact, if some of the Pleistocene chronologies are used, the pass is embarrassingly low!

The hypothetical Modena River provides the water-level stabilization necessary to account for the substantially uniform conditions during deposition of the yellow marl, explains the deep incision of Clover Valley and Meadow Canyon,[15] and may have a definite bearing on the still-unsolved problem of the age and genesis of the present course of the Colorado River.[16] Additional extrapolation, toward mid-Pleistocene, indicates that the Bonneville Basin may not have been closed until about 500,000 years ago, so that the area could not have contained a great lake during the early Pleistocene glaciations, and the Sevier River, which now evaporates in Sevier Lake, could have flowed outward to the sea through such a gap. Theory and observations are in apparent concordance here, for there exists no evidence of any very high lake stage in the Bonneville Basin prior to about Illinoian time.

THE SOUTH RIM PASSES

On the south rim of the Bonneville Basin, between Crestline Pass and the west flank of the

[§] A number of interrelated studies in the vicinity of Thermo show one or more transverse bars on the valley floor, indicating that the Escalante Desert was isolated from the main lake at times.

Markagunt Plateau, in which the multiple canyons of Zion Park are eroded, are three groups of passes (Fig. 4, pass index, 12, 13, 14), all eroded into a complex basement of mixed volcanics and sediments, which was formerly capped in its entirety by lavas of early and middle Tertiary age. These passes are quite complex, the westernmost, to which the field name "Hebron Labyrinth" was applied, having outlets to both the Muddy and the Virgin drainages. The central pass, through Mountain Meadows, site of a famous massacre, is apparently a residual portion of an ancient river, much like Dutton's Kaibab River. The easternmost pass, now traversed by the main road from Cedar City to Zion National Park, has all the earmarks of an ancient outlet from the lake basin except that it is now, and was in the past, by any rational application of modern geological principles, more than 700 feet too high.

THE STANSBURY PROBLEM

The foregoing investigation of outlets shows that about one half the high-water stages can be related to the history of Red Rock Pass, as outlined by Gilbert. Some hope of accounting for the remaining high-water stages, prior to those controlled by the Red Rock Pass outlet, is given by the apparent warping of the basin floor in the southwestern part, leading to a possible outflow in the vicinity of Crestline.

This same evidence, however, shows plainly and convincingly that there is no possibility of an outlet at the Stansbury Level, about halfway from the Provo Level to the present level of Great Salt Lake (*ca.* 4,200 feet above m.s.l.), unless recent and radical changes took place in the Bonneville Basin without leaving any trace. Such changes being improbable, the best present explanation of the Stansbury water-level stability seems to be an evaporational hypothesis. At and near the Stansbury shore line, which is present only on high vertical cliffs, the average gradient of the lake shore is slight. Computations show that in parts of this area a raising of the lake level by only one foot would triple the volume of water contained and would quadruple the area. In consequence, because of this "bathtub effect," a shore line would be incised on vertical cliffs, but not upon gentle slopes; and minor changes in inflow, by a factor such as two, would have a negligible effect on the water level.

Restudy of the Bonneville Basin suggests that Gilbert "builded better than he knew," for not only is the outlet described by him shown to be the one

MAP INDEX

Map	Area	1	2	3	4	5	6	7	8	9	10	11	12	13	14
A	×	×	×	×	×	×	×	×	×						
B	×				×	×	×	×	×	×					
C	×										×				
D	×											×	×		
E	×										×	×	×		
F	×										×				
G	×								×	×		×	×		
H	×								×						
I		×		×		×									
J										×		×	×	×	×
K	×									×	×	×	×	×	×
L														×	×
M				×		×	×					×	×	×	×
N											×	×	×		
O											×	×	×		
P										×	×	×			
Q										×					
R		×		×	×										
S		×													
T										×					
U									×	×					
V												×			
W		×	×	×	×	×	×	×	×	×					
X											×	×	×	×	×

of greatest importance, but other evidence in his report offers a possible solution to a problem unknown to him: that of earlier outlets.

At the present time, with evidence now available, the Modena River and the "bathtub effect" are working hypotheses which may hold the answer to the problem of early outlets and the equally complex puzzle of stabilization of water levels at the Stansbury shore line. Present evidence suggests that the described effects could happen: only extensive additional field work, in many areas, will show whether they did, or did not, happen.

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THE INEVITABILITY OF SCIENTIFIC DISCOVERY

AARON J. IHDE

Dr. Ihde (Ph.D., Wisconsin, 1941) is associate professor of chemistry and integrated liberal studies at his alma mater. His research has been in food chemistry, but he has long been interested in the relationships between scientific and cultural developments. His article is from an address presented at the Annual Meeting of the Wisconsin Academy of Sciences, Arts and Letters, April 24, 1948.

THE history of science reveals numerous instances of discoveries made simultaneously by two or more independent workers. At times this circumstance has resulted in violent battles over claims of priority on the part of the supporters of the opposing parties, as witness the discovery of the calculus by Newton and by Leibnitz, and the extensive litigation resulting from the simultaneous discovery of the telephone by Bell and by Gray. At other times this circumstance has resulted in the manifestation of some of the finer instincts of mankind, as in Alfred Wallace's gracious bows in the direction of Charles Darwin after both had independently arrived at an evolutionary theory based upon natural selection.

Ogburn and Thomas (1922) tabulated a list of one hundred forty-eight instances of such simultaneous independent discoveries taken from the fields of mathematics, science, and technology. In many of their examples more than three independent workers attained the same goal within months of one another. In some cases their examples are open to question, but this in no way decreases the import of their compilation. They list nine men as possible inventors of the telescope, though there is now general agreement that some of these workers did not arrive at the idea independently but obtained it by diffusion of gossip from lens shop to lens shop. Galileo, at least, is recorded as having heard of such an instrument before he constructed his own.

Such instances as these suggest the hypothesis that when the time is ripe a certain discovery becomes inevitable. This thought leads at once to the question, "What constitutes ripeness?" A number of answers suggest themselves. The theologian is apt to suggest that a benevolent deity has decided to pass this blessing on to mankind. Such an answer is unsatisfactory to the scientist, who insists on finding causal relationships more closely associated with the problems immediately at hand.

The answers that suggest themselves to the scientist are:

1. A need has arisen, so a search is undertaken to fill that need.
2. The genius with the proper insight into the problem has reached his maturity.
3. The fundamental background of knowledge necessary for the discovery has been accumulated.

The first answer has as its basis the oft-heard remark that "Necessity is the mother of invention." There is basis for the belief that an expressed need will result in an effort to satisfy that need, but this is only part of the picture. As Ogburn (1922) said in this connection, "In earlier times the necessity for quicker transportation, or a more stable food supply, or methods of preventing the death of babies was perhaps more urgent than now, but such wants did not produce inventions." The needs of the Alexandrian astronomers for a more refined tool for studying the movements of the planets did not produce the telescope. The need for painless surgery did not bring about the early use of anesthesia. The need for an instrument to study the chemical composition of the stars did not bring about the development of the spectroscope.

Obviously, necessity is no more than a minor factor in bringing about discoveries and inventions. Necessity can only direct attention toward problems that require solution but cannot bring about their solution if certain other things are lacking. Let us pass on to the second proposal.

Much popular writing about scientific discovery has pointed to the superior intellect and ability of those making great discoveries. According to this school, mankind would still be struggling along without benefit of the Mendelian laws of heredity had Mendel never lived, or without the germ theory of disease had there been no Pasteur, or without X-ray had Röntgen died in infancy. In spite of the enthusiasm of the popularizers, there seems to be little question but that all such knowledge would now be available to us. The large number of times that important discoveries were made independently by another scientist rules out the thesis that a certain person was essential to

discovery. True, the discovery might not have been recognized quite as soon, but recognition was certain to come.

Mendel's discovery, in fact, failed to receive recognition for thirty-four years following publication, partly because the journal in which he published was not widely distributed, partly because biological thought was in the throes of controversy over Darwinism. Three botanists, De Vries of Holland, Collens of Germany, and Tschermak of Austria, actually rediscovered the Mendelian principles and published within several months of one another in 1900. The fact that all three had become aware of the existence of Mendel's paper before they published does not nullify the importance or originality of their own work. All of them should have arrived at the fundamental laws had Mendel's paper been nonexistent.

Similarly, the calculus did not require an Isaac Newton. Leibnitz made the same discovery. Had there been no Newton or Leibnitz, it is conceivable that another mathematical thinker could have succeeded. The argument can be advanced that only a mind such as that of a Newton or a Leibnitz was capable of comprehending such a concept. One may ask whether the mind of Euler, Lagrange, Laplace, Gauss, or a Bernoulli might not have succeeded with the fundamental background then available.

Kroeber (1917) has stated,

> . . . no one can sanely believe that the distinction of Darwin's greatest accomplishment, the formulation of the doctrine of evolution by natural selection, would now stand to his credit had he been born fifty years sooner or later. If later, he would have been infallibly anticipated by Wallace, for one thing; by others, if an early death had cut off Wallace. That his giant restless mind would have evolved something noteworthy is as likely as it is away from the point: the distinction of the particular discovery which he did make, would not have been his. Put on earth, by contrary supposition, a half century earlier, his central idea would not have come to him; as it failed to come to his brilliant predecessor, the evolutionist Lamarck.

One is led to question, further, the alleged need of a certain mind for a certain discovery. Although there is little question that discoveries are seldom made by those of inferior intelligence, there have been numerous instances of developments made more as a result of persistence than of mental brilliance. The record is filled with the names of those who made only a single important discovery. If their intellects were so extraordinary should they not have continued to make great contributions? Whenever, on the other hand, their work has been extensive they have generally contributed their full share of erroneous ideas.

Added to this is the fact that many important discoveries were completed while the worker was still a comparative youth. The U. S. Patent Office once published a "Table of Important Inventions." Of a total of eighty-five inventions, only two were made by men who had passed their sixtieth birthday, only eight by men in their fifties. Forty-three of these inventions were credited to men who had not yet reached thirty-five. Genius or learned background can hardly be the answer.

This brings up the third proposal, that discovery becomes inevitable when the fundamental background for the discovery has been laid. Here solid ground appears. Regardless of the need, and given an academy full of geniuses, a discovery remains impossible as long as any of the fundamental background is missing. Once the last bit of background has been accumulated, the discovery becomes possible. It may not be made at once, but its ultimate solution is sure to come.

Let us consider the atomic theory of John Dalton in this connection. Ideas about atoms were part of the philosophical repertoire of some of the early Greeks, but such a theory could not gain precedence over a theory of the continuity of matter until it could explain experimental observations better than a theory of continuity was able to explain such observations. Such an opportunity did not present itself until chemistry became a well-established science about 1800. By then the modern concept of elements was established, the analytical balance was in common use, the law of conservation of matter was generally accepted, the law of definite proportions was being debated, the law of partial pressures of gases was being discovered, and the law of multiple proportions was becoming apparent. The time was ripe for one steeped in the corpuscular philosophy of Newton to arrive at the idea of chemical atoms and attribute characteristic weights and properties to them. In fact, enough background had already been laid fifteen years before for William Higgins to arrive at essentially the same conclusion.

Although the chemical background of the first half of the nineteenth century led to a belief in atoms, it was not yet ready for an atom composed of parts. Such an atom had to wait until the atomic concept of Dalton had first become firmly established and then proved inadequate to explain new chemical and physical phenomena. In the last half of the century such inadequacies became glaring enough to bring about revision of atomic concepts leading to a particle composed of protons and electrons.

Discovery of the elements gives us another ex-

ample of the need for background before discovery becomes possible. Seven metals and two nonmetals were known to the ancient world. The last gaps in the periodic table of some ninety elements have been filled only recently. The discovery of the elements has followed very closely the increased understanding of new chemical principles. Those elements known to the ancients are inactive enough that they either occur in nature in the elemental form or are easily reduced to the element in a hot charcoal fire. New elements were not added to the list until recent centuries. The alchemists, using chiefly heat and charcoal, were successful in the preparation of elemental arsenic and antimony. The more difficult reduction of phosphorus was also finally achieved by these methods. Isolation of the remainder of the fairly abundant metals had to await the development of mining and metallurgy to the point where marginal ores were becoming of interest, and the development of chemical operations to the point where fairly refined separations were possible. The gaseous elements were not discovered until satisfactory methods of collecting gases were devised. The very active metals, such as potassium and sodium, could not be discovered until the work of Galvani and Volta in the field of electricity brought the chemist a new tool in the form of electrolysis. With the isolation of sodium and potassium, the discovery of calcium, magnesium, strontium, and barium naturally followed in short order. The availability of potassium made it possible for Oersted and Wöhler to prepare metallic aluminum. Preparation of this metal by an alternative procedure was not possible for half a century, until knowledge of electrolytic phenomena and the solubility behavior of aluminum compounds had advanced to the point that Hall in America and Héroult in France independently discovered the basis for the present commercial process.

The development of the spectroscope in the hands of Bunsen and Kirchhoff made possible the discovery of the elements rubidium and cesium, which are so closely related in properties to sodium and potassium. Their preparation by the electrolytic method of Davy would have been simple. Their scarcity, however, made necessary the invention of a new analytical tool before their discovery was possible. The spectroscope also made possible the discovery of a number of additional elements, including the rare gases, argon, helium, and neon. These gases had come to the attention of Cavendish more than one hundred years before. Cavendish, careful and exhaustive worker that he was, lacked both the mental and instrumental background available to Ramsay and Rayleigh a century later.

Most of the radioactive elements, likewise, had to await methods for their detection. Once the existence of radioactivity phenomena was apparent and methods for the detection of radioactivity were worked out, progress was dramatically rapid. This rapid progress once again resulted in a number of discoveries made at almost the same time in independent laboratories.

Finally, the discovery of the transuranium elements became possible only with the discovery of accelerating devices, atomic transmutation, the neutron, and artificial radioactivity. No one in his right mind would have embarked, fifty years ago, on a deliberate search for an element beyond uranium in the periodic table. Science does not progress in that fashion. It moves forward along unpredictable paths for the most part. True, a nation can set forth upon a project to develop an atomic explosive, but only after a sufficient amount of background has been laid to point to the possibility of such a development. The need for such an explosive only speeded up the search. The development, nevertheless, was inevitable, as is all scientific discovery.

Thus, we are led to conclude, as the result of an all-too-short survey, that the primary factor in bringing about scientific discovery is not necessity, or individual genius, but the relentless pressure of accumulating knowledge. Seldom is it possible to foresee all those factors that are essential to the solution of a certain problem until they have accumulated and become a part of man's scientific heritage. Once the accumulation is complete the next step becomes inevitable.

SOUND WAVES AND RHYTHMS

VERN O. KNUDSEN

Winner of the Annual Thousand Dollar AAAS Prize in 1934 for his paper on "The Absorption of Sound in Gases," Dr. Knudsen (Ph.D., Chicago, 1922) is professor of physics and dean of the Graduate Division, University of California, Los Angeles. His article is from an address presented in the symposium on "Waves and Rhythms" at the AAAS Centennial in Washington, September 13–17, 1948. Almost immediately after that meeting, Dr. Knudsen sailed for Europe to make a study of the acoustics of such famous concert halls and opera houses as those in Stockholm, Gothenburg, Oslo, Copenhagen, Amsterdam, and Milan. He will also visit the Roman theatres in southern France.

THE rhythmical patterns of sound waves in air—music, speech, and noise—surround us eternally, and we hear them from the cradle to the grave. Sometimes these rhythmical waves flow gently and sweetly over us, and gladden our hearts; sometimes they impinge harshly on our ears, and disturb our sleep or raise our blood pressure. Most of what we learn and much of what we enjoy are conveyed to us on waves of sound. It is natural, therefore, that man should be curious about these waves and rhythms, as indeed inquisitive man always has been.

Although the art of music was developed by the Chinese, Egyptians, and Hindus at least six thousand years ago, the systematic study of the rhythmical waves of music began with Pythagoras, some twenty-five hundred years ago. By means of the monochord he established the basic relations between consonant musical intervals and the ratios of simple numbers. He discovered that two segments of the monochord gave the perfect consonance of the *octave* when the ratio of the lengths of the two segments was 1:2, the consonant *fifth* when this ratio was 2:3, the *fourth* when 3:4. Pythagoras and his disciples maintained, with characteristic Greek logic, that "the intervals of music are rather to be judged intellectually, through numbers, than sensibly through the ear." But Aristoxenus, some two hundred years later, espoused the view that the ear is the sole and final authority for determining consonance or dissonance. The battle between these two schools continued for many centuries (it flares up even in the twentieth century); each school held the other in contempt, and, as in too many later controversies in art and science, each was more concerned with its own triumph and the bowling over of its opponent than with the finding of the truth. Although each had discovered a part of an eternal truth, modern acoustics favors the views of Aristoxenus.

We have come a long way during these past twenty-five centuries in developing and exploiting our accumulated knowledge of the vibrations, waves, and rhythms that characterize sound. Until recently, this knowledge was concerned almost exclusively with music. Bacon recognized this limitation when he said, "The nature of sound hath in some sort been enquired as far as concerneth music."

The science of sound really began with Galileo, who is much better known for his epochal discoveries in mechanics and astronomy, and for his cherished recalcitrance in heeding the edicts of cardinals and the verdict of the Inquisition. In Galileo's *Two New Sciences* (1638), he deduces the laws of vibrating strings, showing how the frequency of vibration depends upon the length, diameter, density, and tension of the string. He confirmed the findings of Pythagoras, discovered the well-known relation between pitch and frequency (which, like so many other discovered laws in nature, is only an approximation), explained resonance, and described the causes of consonance and dissonance.

Although Vitruvius (about 50 B.C.) described certain acoustical properties of theatres, such as echoes, reverberations, and interference, Kircher (1650) was probably the first to apply the principles of geometrical acoustics to the design of the shape of rooms. He anticipated the focusing effect of sound in rooms having cylindrical or elliptical ceilings, as occurs, for example, in the Salt Lake Tabernacle.

Many of the rhythmical properties of sound were discovered by Chladni's researches on the nature of the vibration of strings, rods, and plates, published

in *Die Akustik*, in 1802. Almost every freshman student of physics has seen the beauty of Chladni's sand figures, which reveal the infinitely many modes of vibration of a bowed plate.

Many advances in the acoustics of the eighteenth century were made possible by the newly developed calculus. The vibrating string, especially, but also vibrating pipes and elastic fluids, became the proving ground on which such distinguished scientists as Euler, Lagrange, and D'Alembert tested new differential equations that have proved so fruitful in many branches of classical and modern physics besides acoustics.

The epochal advances in the nineteenth century are too numerous even to catalogue in this brief backward glance at the evolution of acoustics, but I cannot refrain from calling the roll of honor of such great contributors as Biot, Fourier, Savart, Ohm, Henry, Tyndall, Stokes, Kirchhoff, Lissajous, Helmholtz, and Rayleigh. Rayleigh's *Theory of Sound*, first published in 1877, and revised in 1894, is a compendium of the important acoustical works of the nineteenth century and before, but it is vastly more. It is a record of his own monumental contributions, which touched and elevated almost every branch of acoustics. It is no less than the "Principia of Acoustics." It continues to be, even in 1948, the standard book in its field. Most acoustical investigations of the twentieth century may well begin with references to what Rayleigh contributed. He treated most phases of the subject so thoroughly that many physicists of two or three decades ago felt that nothing of importance remained to be explored in the field of acoustics. There was indeed a period of low interest and activity from Helmholtz' *Tonemfindungen* (1863) and Rayleigh's *magnum opus* until the advent of the condenser microphone and the thermionic vacuum tube (about 1915) ushered in the modern era of acoustics. During this era telephony, radio, architectural acoustics, psychoacoustics, ultrasonics, and many other fundamental and technical aspects of acoustics have developed in magnitude and complexity to the extent that no one today would attempt to become an expert in all branches of acoustics. There are more than 1,400 members in the Acoustical Society of America, and during the past year the *Journal of the Acoustical Society* published six issues, numbering 875 pages.

I shall here review briefly, and in a nontechnical manner, some of the progress that has been made in recent years by certain investigations of (*a*) the propagation of sound waves through the atmosphere, and (*b*) the behavior of sound waves in rooms.

Sound waves and rhythms in the atmosphere. A few exceptional sounds, such as that due to the violent eruption of Krakatao in 1883, or to the Great Siberian Meteor of 1908, excite the entire atmosphere surrounding the earth into its resonant or normal modes of vibration. The theory of these atmospheric vibrations was first developed by Laplace (1845), and was extended by Lamb, Solberg, Taylor, and Pekeris. The period of the waves set up in the atmosphere by the explosion of Krakatao was of the order of one hour; the period of those excited by the Siberian Meteor was of an order of two or three minutes. Such slow oscillations are propagated with almost no attenuation—the subsonic aerial oscillations of Krakatao, as revealed by microbarographs in London, Berlin, Palermo, and Valencia, traveled around the globe four times. This blast continued on its eastward and westward stratospheric journeys for ten days before it spent its force.

But most familiar sounds—the whistle of a train, the peal of a church bell, or the song of a bird—are propagated at most a few miles, and soon become inaudible, too feeble to be detected by the most sensitive acoustical instruments. Such sounds are confined to small regions near their sources, and thus do not sensibly excite the normal modes of vibration of the entire atmosphere. The paths along which these sounds travel in the air are determined largely by the distribution of temperature and wind in the atmosphere. The distances they travel along these refracted paths before they are dissipated into heat depend upon the frequency of the sound, upon the temperature, humidity, viscosity, and heat conductivity of the air, and upon the nature and amount of fog and smoke in the air. Stokes and Kirchhoff calculated the attenuation of sound waves owing to viscosity and heat conductivity. Their findings indicated that such losses were negligible for most audible sounds, but that the attenuation increased with the square of the sound frequency. There are three other types of losses, however, that were not anticipated by Stokes and Kirchhoff, and these losses often are the predominant ones, especially for sounds in the audible frequency range. The most important of these three types of losses is one due to a lag of pressure, with respect to the condensation of the sound wave, which results from the transfer of energy during collisions between translating and vibrating molecules. At temperatures above about 0°C, oxygen molecules are capable of being excited into vibration as a result of molecular collisions, and collisions between oxygen and water vapor molecules are especially effective in "exciting" or "de-exciting"

the vibration of the oxygen molecule. The number of vibrating oxygen molecules waxes and wanes as successive condensations and rarefactions of the sound wave pass through the air, but the process is not a reversible one, and thus a part of the energy of the sound wave is converted into heat. The attenuation is negligible at very low frequencies because the loss of sound energy during each cycle of the sound wave is very small; the attenuation is negligible at very high frequencies because the entire cycle of the sound wave occurs in a time that is short compared with the time required to establish equilibrium between the vibrating and translating oxygen molecules; but the attenuation becomes a maximum, and may be a hundred times as great as that due to viscosity, when the period of the sound wave is of the same magnitude as the time required to establish equilibrium. This source of the attenuation of sound waves was scarcely suspected, and certainly not comprehended, only twenty years ago. At that time, it was taken for granted that the velocity of sound was independent of the frequency of the sound wave, and that the degradation of the energy of the sound wave into heat depended primarily on the viscosity and heat conductivity of the air.

The large effect of temperature and humidity on the attenuation of sound in air is indicated by a comparison of the audibility of sounds in the Arctic and in a desert. The cold winter air of the Arctic is acoustically transparent; the attenuation of sound is primarily that attributable to viscosity and heat conductivity, which is very small at the lower audible frequencies. In terms of the collision theory, the vibrational heat capacity of oxygen at such low temperatures is insufficient to contribute any attenuation from energy exchanges during molecular collisions. Under such conditions, barking dogs and crowing roosters have been heard at distances of ten or more miles. In contrast, in the hot summer air of the desert, such as at Greenland Ranch, Inyo County, California, when the temperature reaches 134° F., and the relative humidity declines to 2.4 percent, the attenuation at 6,000 cycles per second is 30 db per 100 yards. Thus, a sound wave of this frequency, in such desert air, has 99.9 percent of its energy degraded into heat for each 100 yards the sound wave advances; the same sound wave in the cold Arctic air would travel at least 3,000 yards before it lost this same fraction of its energy.

The presence of fog or smoke in the air contributes further to the attenuation of sound. When the water droplets or the smoke particles are relatively large, they remain at rest with respect to the much smaller molecules of air that are forced into vibration by the sound wave, and thus sound energy is lost by the viscous drag between the suspended particles and the air. When the particles are very small (or when the frequency of the sound is sufficiently low) the particles participate in the forced vibratory movement of the medium, and thus the viscous force is diminished, or even vanishes, for sufficiently small particles. When the particles are water droplets, as in fog or clouds, there is another source of attenuation of sound that is attributed to the periodic evaporation of water from, and its recondensation on, the water droplets. This periodic evaporation and condensation follows the periodic heating and cooling of the pressure variations of the sound wave. This is a "relaxation" type of attenuation, which results from the lag of the evaporation and recondensation behind the periodic changes of temperature induced by the sound wave. It is responsible for most of the observed loss of energy of a sound wave in fog for frequencies below about 500 cycles and, according to recent measurements in our reverberation chamber, may amount to as much as 1 db per 100 yards in a dense fog. No such loss is observed in a fog of oil droplets, although at higher frequencies where the viscous forces are predominant, fogs of oil are as effective as are fogs of water for attenuating sound waves. For example, at a frequency of 6,000 cycles, a water fog having a density of 2×10^{-6} grams per cubic centimeter (visibility of about 100 feet), the attenuation was about 3 db per 100 yards, and for an oil fog of about two thirds the same density and slightly smaller particle size, the attenuation was about 4 db per 100 yards.

Thus are the waves and rhythms of sound in the free atmosphere degraded into heat. The viscosity and heat conductivity of the air, the delays involved in the exchanges of vibrational and translational energy between the colliding molecules of oxygen and water vapor, the viscous drag between suspended particles and the vibrating air surrounding these particles, and the evaporation from, and recondensation on, the water droplets of fog and clouds, all exact their toll of energy from sound waves propagated through the atmosphere.

Sound waves and rhythms in rooms. Although many of the dissipative forces just considered are operative on the waves and rhythms of sound in rooms, and in large rooms contribute significantly to the absorption of high-frequency sounds, the boundaries of these rooms are largely responsible for the character of the sounds we hear in them. The shape and size of a room, and the materials

that comprise its boundaries, determine the acoustical properties of the room. Every enclosed space is an acoustical resonator, in which there is a triply infinite number of resonant frequencies. In a rectangular room, $55' \times 100' \times 20'$, the lowest or gravest resonant frequency is one that corresponds to the fundamental of an organ pipe 100 feet long, closed (or open, neglecting end effects) at both ends, namely, about 5.6 cycles per second. Other resonant frequencies for this room are, in ascending order, 10.3, 11.3, 11.7, 16.9, etc. cycles per second. There are some 440 resonant modes in this room having frequencies below 110 cycles, about 440,000 below 1,100 cycles, and countless others at higher frequencies. All these resonant frequencies are excited when sounds are produced in such a room. Sometimes one or several of them may be more prominent than the others, in which case the room may have a peculiar resonance. A tiled rectangular bathroom is an example familiar to most of us. We often are deceived about the resonant quality and bigness of our voice as we test it in the bath. The room rather than our voice is the musical instrument we hear—our voice may be little more than the bellows that makes the room "speak" or "sing" at its resonant frequencies.

When a radio, violin, or any musical instrument is played in a room, that room is, in effect, a part of the instrument; we say that the instrument is *coupled* to the room. The instrument excites the normal modes of vibration of the room, and if some of these are too prominent they mar the tonal quality of the instrument as heard in that room. A high-quality radio or a world-famous Stradivarius cannot produce high-quality music in a room that has faulty resonance, or in any room that suffers from other acoustical defects.

One of the prime requirements for good acoustics in a room is the proper control of these resonant frequencies. The shape of the room should be designed so that its resonant modes of vibration are as uniformly spaced as possible; that is, so that several modes do not have the same, or even very nearly the same, frequency. A cube, for example, has the same fundamental frequency for its length, breadth, and height, and the harmonics for each of these three fundamental modes are respectively the same. In such a cubical room, especially if it is small with highly reflective boundaries, there will be prominent resonances at audible frequencies that coincide with the frequencies of these fundamental and harmonic modes. Similarly, rectangular rooms whose dimensions form ratios of 1:2:3, 2:3:4, or other ratios of small whole numbers, are more likely to have peculiar resonances than are rooms whose dimensions have incommensurate ratios.

More important than these ratios, for the control of resonance and other relevant acoustical properties of a room, are the selection and location of absorptive materials for the boundaries of the room. A small room with its floor carpeted and its ceiling lined with acoustical tile may, contrary to popular opinion, suffer from resonances. If the walls are smooth, unbroken, and highly reflective, certain low-frequency modes that vibrate tangentially to the floor and ceiling will be only slightly damped, and the room therefore may be excessively resonant and reverberant at the frequencies of these tangential modes. A room so treated may contain more absorption than is required for good acoustics; but, by reason of the improper placement of the absorptive materials, the room will have poor acoustics unless the walls have been treated so as to contribute a high degree of diffusion of sound. Absorptive materials should be distributed over the boundaries of a room in such a manner that all modes of vibration are damped approximately the same amount. This usually requires the careful selection and placement of reflective and absorptive materials for the walls as well as for the floor and ceiling of a room. In general, the materials should absorb all audible frequencies—and not merely high ones, as is characteristic of thin carpets and many commercial materials—and they will be more effective if distributed in patches, strips, or panels than if concentrated in a few large areas. When distributed in patches over the walls and ceiling, such absorptive materials not only ensure uniform damping of all the resonant frequencies of the room, but they also promote a desirable diffusion of sound. In such a room steady-state sounds are uniformly distributed, and transient sounds build up and die away in a smooth, pleasing manner, thus contributing to good acoustics.

There is reason to believe that the hearing of speech in large auditoriums may be improved by a combination of (1) acoustical treatment in which the low frequencies are absorbed more than the high ones, and (2) sound amplification in which the high frequencies are amplified more than the low ones. Most absorptive materials used for the reduction of reverberation in rooms absorb the high-frequency consonants more than they do the relatively low-frequency vowels. In such rooms the vowels die away more slowly than do the consonants, and therefore they often mask the much feebler consonants that follow the vowels.

Measurements of the hearing of speech in a

reverberant auditorium revealed that 93.3 percent of the vowels were heard correctly but only 76.4 percent of the consonants were so heard. Of the total number of 955 consonant errors in these tests, 529 were final consonants, 224 were initial consonants, 117 were omissions of the called consonants, and 85 were erroneous additions of consonants that were not called. Thus, final consonants were mistaken more than twice as frequently as were initial ones. This is convincing evidence that the masking of a final consonant by the reverberation of the preceding vowel is responsible for many errors in the recognition of the sounds of speech in rooms.

It seems worth while, at any rate, to investigate the effect on the hearing of speech in a room of (1) treating the room with absorptive material that is at least two or three times as absorptive at low frequencies as it is at high ones, and (2) reproducing the speech by means of an amplifier whose gain, or amplification factor, increases about 3 or 4 db per octave throughout the frequency range from about 125 to 8,000 cycles.

There are other means for improving the hearing of speech in rooms that are suggested by the results of speech articulation tests: (1) a radical means, namely, to devise a system of speech that is free from final consonants and from other frequent sources of error; or (2) a more moderate and sensible means, namely, to train all speakers to emphasize those sounds of speech that give the greatest difficulty, especially such consonant endings as *ng, d, v, f,* and *th,* which are responsible for a large portion of the complaints concerning poor acoustics in rooms.

Returning to the problem of the acoustical design of rooms, the mathematical difficulties of analysis of the growth, decay, and steady-state of the triply infinite modes of vibration in a typical room are well-nigh insuperable. The methods of physical or wave acoustics are the only rigorous ones, and these have been used to calculate the behavior of a few low-frequency modes in rectangular rooms, and a few other regular shapes, when the reflective and absorptive materials are simply distributed. The formulas of physical acoustics become so complicated, however, for actual rooms in which the boundaries are nonuniform, and in which it is necessary to deal with a large number of modes, that it is not yet feasible to expect architects and engineers to use design formulas based on physical acoustics. Even those who are familiar with the mathematical complexities of physical acoustics are confronted with the most formidable tasks of computation; the calculations for a relatively simple treatment of a rectangular room would require weeks of routine labor even with the help of computing machines.

The methods of geometrical, *or ray,* acoustics avoid these complications. Although only approximate for dealing with most problems in architectural acoustics, and utterly misleading for some others, these methods, when one is guided by an understanding of their limitations and by the general principles of physical acoustics, are extremely useful. Fortunately, these methods are adequate for the acoustical designing of most rooms. Strictly, however, geometrical acoustics is applicable only when the wave lengths of the sound are small compared to the dimensions of the room; when the wave lengths are sufficiently short, the simple formulas of geometrical acoustics become good approximations to the more precise formulas of physical acoustics. For the design of rooms of moderate size, geometrical acoustics is satisfactory for frequencies above about 250 cycles, and for large auditoriums it is a good approximation to physical acoustics at all frequencies of practical interest. But for the design of small rooms, especially those that are to be used for music or for broadcasting and recording purposes, recourse to physical acoustics should be made, at least qualitatively, to the fullest possible extent. When this is done, acoustical designing becomes much more than "acoustical correction" by the application of absorptive materials to the ceilings of rooms, important though this may be in many cases for the control of reverberation and the suppression of noise.

Good acoustical designing begins with the first preliminary sketches for a building. It involves such considerations as the selection of a suitably quiet site, or, if the site is a noisy one, the use of sound-insulative constructions that will eliminate all undesirable noises; it requires the design of a room shape that, on the one hand, will avoid echoes, prominent reflections, sound foci, and room flutter, and, on the other hand, will ensure proper diffusion and the most advantageous flow of sound to all auditors in the room; it involves the selection and distribution of reflective and absorptive materials and constructions that will provide the optimum conditions for the growth, decay, and the steady-state of sound in the room; it requires, if the room is a large auditorium or theatre, the installation of a high-quality sound-amplification system; it requires inspections during construction, and tests of the completed structure, to make sure that the specified sound insulation and sound absorption have been realized; and, finally, it re-

quires even that maintenance instructions be left with the building manager, indicating how the acoustical materials can be cleaned or redecorated, which furnishings in the building must be retained in order to maintain good acoustics, how the humidity should be controlled (in large rooms where the highest standards are desired) in order to avoid excessive absorption of high-pitched sounds, and how the sound-amplification system should be operated and maintained. If the foregoing steps are intelligently and skillfully executed in the design and construction of an auditorium, there need be no anxiety about the acoustical outcome—the acoustics of the room will be good. The waves and rhythms of sound in such a room will be undisturbed and undistorted; they will be properly enhanced and will fall gently on the ears of the auditors.

WINTER WIND

Save for a white asepsis
His unattended birth
Is marked less by the crash
Of mountains' ice,
Than silences that swell
Surround and drown
The artifacts of air and sound

An alien in an alien land,
He roams—
As restless as the mind—
The tundra flats and prairies,
Then pinnacles,
To test his might on boulders
Stranded after snow.

Soon he will call;
First, begging at your windowpane,
Then slinking to the floor.
He'll whine;
He'll roar like surf against your door.
By timber turned,
He'll flee into the night.

He'll sweep the waters of the sea
And land, the sand.
He'll fracture saplings, crash down oaks
And in his frenzy spend himself
Against his sibling foe,
The glacial boulder,
Whose thrust he cannot turn.

JOSEPH HIRSH

THE NEANDERTHAL CONTROVERSY: NINETEENTH-CENTURY VERSION

JACOB W. GRUBER

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IN 1848, just a century ago, a human cranium of unusual visage was unearthed during quarrying operations on the Rock of Gibraltar. Until then, no recent man had ever looked upon the face of his Ice Age predecessors—the Neanderthaloids—that massive-browed and flat-headed race of men who roamed the continent of Europe as its only inhabitants, when the last of the glaciers first poked its icy lobes toward the Mediterranean. In the long span of geologic history, a century is not a very long time; but measured in terms of scientific accomplishment the past hundred years encompass almost the entire history of human paleontology.

In that hundred years, the Neanderthaloids have not only provided the most numerous remains of Pleistocene man, but they have, in addition, presented the searchers for human origins with some of their knottiest problems. Having as their habitat the area in which modern Western civilization developed, they have been the subject of some of the most violent controversies in human paleontology to determine whether such a brutish form could be ancestral to today's rulers of the universe. Their true place in the evolution of man has never been completely established. Do they represent the direct ancestor of modern man, or merely a distant cousin who dropped off the line of human evolution at some far-off time in the past? Do they represent specimens of normal individuals, or are they only pathological deviations from the normal stock? Are they a race, a species, or genus of man, or are they man at all? These are the questions inherent in the Neanderthal form and his distribution. As newer discoveries have been made, as more and more information has come to light concerning Pleistocene man, the problem of the Neanderthaloids becomes more and more confused. Today, particularly, with the apparent substantiation of a *sapiens* type in Europe prior to the Neanderthaloids, the problem of the place of Neanderthal man in human evolution requires a re-examination, a re-examination unclouded by strongly held tenets of another day. Almost lost in the present confusion regarding the true nature of this form is the bitter scientific controversy that raged over the first specimens. Perhaps it is not amiss, under such circumstances, to re-create certain aspects of that debate and the scientific atmosphere under which it was waged.

With imaginations and intellects chained by too-literal interpretations of the Scriptural record, which maintained a recency of 6,000 years for the creation of man, the participants in the burgeoning sciences of the eighteenth and early nineteenth centuries were unable even to conceive of earlier forms of man in either a chronological or a morphological context. Certainly, the brilliant though cautious Cuvier, acknowledged master of the natural scientists a century and a half ago, contributed greatly to the limitations, intellectual and otherwise, imposed upon those who sought for the origin of man in a remote antiquity. Revolutionary and radical in the fields of comparative anatomy and tertiary paleontology, but extremely conservative regarding questions of man's origin, he reiterated time and time again the religio-scientific dogma of man's recent creation and the improbability, if not impossibility, of ever finding his remains earlier than the most recent times. Cuvier's fame and unquestioned skill in his own field strengthened the doubts and skepticism that permeated the intellectual atmosphere of the first half of the nineteenth century, an atmosphere so clouded with a priori judgments as to condemn, almost without hearing, conclusions based on scientific investigations of undoubted merit that tended to imply that man coexisted with extinct glacial fauna.

As early as 1797, John Frere addressed a short communication to the Society of Antiquaries of London, in which, after summarizing his find of some flints, obviously fashioned by man in a stratum underlying the bones of extinct fauna, he very cautiously suggested that "the situation in which these weapons were found may tempt us to refer them to a very remote period indeed; even beyond that of the present world." Frere's suggestion, however, was heard only to be forgotten. It was not until almost half a century later that increasing

evidence, supplied during the intervening years primarily by the unrecognized labors of such pioneers as MacEnery in England, Schmerling in Belgium, and, above all, Boucher de Perthes in France, made it obvious and intellectually necessary to accept, though in some quarters reluctantly, Frere's views as to the existence of man during glacial times. So overwhelming had the evidence become that in 1859, only twelve years after Boucher de Perthes had set forth the results of his investigations at Abbeville in his celebrated, but scathingly reviewed, *Antiquites Celtiques,* Sir Charles Lyell in a major scientific address could say:

> The facts, however, recently brought to light during the systematic investigations, as reported on by Falconer, of the Brixham Caves, must, I think, have prepared you to admit that scepticism in regard to the cave evidence in favor of the antiquity of man had previously been pushed to an extreme. To escape from what I now consider was a legitimate deduction from the facts already accumulated, we were obliged to resort to hypotheses requiring great changes in the relative levels and drainage of valleys, and, in short, the whole physical geography of the respective regions where the caves are situated—changes that alone would imply a remote antiquity for the human fossil remains, and make it probable that man was old enough to have co-existed, at least, with the Siberian mammoth.

The dramatic change in scientific attitudes respecting the age of man is more vividly expressed in an editorial in the *Geologist* of August 1862, in which S. J. Mackie very candidly remarks:

> When the antiquity of man was first proclaimed from the discovery of the Abbeville flints by Boucher de Perthes, no one believed it. Everybody thought him like the mad man who swore all the world was mad; and so it seemed, then, as if all the world had mental obliquity of vision, which made them declare our *savant* of Abbeville to be labouring under a delusion. When, however, Rigollet, Prestwich, Flower, Lyell, Evans, and others of the goodly company of geologists,—as unbelieving, however, as so many St. Thomases,—went, saw, and returned believing, the fame of Boucher de Perthes' discoveries gained ground. Some there were who hardened themselves in their unbelief, and hazarded wild theories of ocean-waves chipping out artificial forms, and of recent objects sinking down in the ground, and burying themselves, and other equally untenable notions, but these waxed fewer and fewer, not by dying out, but by becoming converts to the novel truths.

Lyell and Mackie, however, represented the "radical" converts to the conclusions of Boucher de Perthes. More in keeping with the thought of the times was the opinion expressed by Prestwich before the Royal Society of London in 1859, an opinion which is curiously akin to those expressed not too long ago by Americanists convinced of the recency of man in the New World. Prestwich

> does not, however, consider that the facts, as they at present stand, of necessity carry back man in past time more than they bring forward the great extinct mammals towards our own times, the evidence having reference only to relative and not to absolute time; and he is of the opinion that many of the later geological changes may have been sudden or of shorter duration than generally considered. In fact, from the evidence here exhibited, and from all that he knows regarding drift phenomena generally, the author sees no reason against the conclusion that the period of man and the extinct mammals—supposing their contemporaneity to be proved—was brought to a sudden end by a temporary inundation of the land; on the contrary, he sees much to support such a view on purely geological considerations.

Shades of Cuvier and his catastrophism!

It was in such a period of doubt and skepticism, of biblical injunctions and science, that the first evidence other than cultural remains of Pleistocene, or glacial, man, the Gibraltar skull, was brought to light. Understandably enough, the notions prevailing a decade before the *Origin of Species,* concerning man's place in nature, served to deprive the find of its proper importance and to hide its true meaning for the development of man. Little is known, therefore, of the early history of the Gibraltar skull, and much of that is due to the later investigations of Sir Arthur Keith.

The find is documented by an entry in the minutes of the Gibraltar Scientific Society, dated March 3, 1848, which merely reads "Presented a Human Skull from Forbes Quarry, North Front, by the Secretary." Its history from 1848 until it was brought to England sixteen years later is unknown. By the time the Gibraltar skull was brought to the notice of English paleontologists in the mid-1860s, a peculiar skull from the Neander Valley of Germany had already been the center of a raging controversy for half a dozen years and, overshadowed by what was later to be proved a fellow, the Gibraltar skull was forgotten again.

Recovered in August 1856 by Fuhlrott, the Neanderthal skull was first brought to the attention of a scientific body by Professor D. Schaaffhausen in a report read to a meeting of the Lower Rhine Medical and Natural History Society at Bonn on February 4, 1857. It was in the course of this paper, published in 1858, that Schaaffhausen gave the first, and one of the few, complete anatomical descriptions of the skeleton, along with the circumstances of its accidental discovery and preservation.

Workers, digging through unstratified loam deposits in a cave opening on the cliff face of the Neander Valley near Dusseldorf, unearthed a group of bones which they thought to be nonhuman. Through the fortunate appearance of Dr. Fuhlrott, who recognized the true nature of the remains before they had been completely discarded, a large portion of the skeleton was preserved. The

parts extant included primarily the skullcap, which had apparently been broken from the base of the skull at the time of disinterment, and several of the long bones. The left humerus was much slenderer than the right, and the coronoid process of the left ulna, or that portion which articulated with the humerus, was so much enlarged by bony growth as to preclude the possibility of the flexure of the elbow beyond a right angle; the anterior fossa of the humerus was filled with the same bony growth. Despite the fact that the remains were some four or five feet below the surface of the cave, no faunal remains or artifacts were found by which an approximate dating could be made.

In his detailed report on the form, Schaaffhausen concluded:

> (1) That the extraordinary form of the skull was due to natural conformation hitherto not known to exist, even in the most barbarous races; (2) That these remarkable human remains belonged to a period antecedent to the time of the Celts and the Germans, and were in all probability derived from one of the wild races of Northwestern Europe, spoken of by the Latin writers; and which were encountered as autocthones by the German immigrants; (3) That it was beyond doubt that these human relics were traceable to a period at which the latest animals of the diluvium still existed; but that no proof in support of this assumption, nor consequently of their *fossil* condition, was afforded by the circumstances under which the bones were discovered.

Schaaffhausen in his anatomical description of the remains remarked on the enlarged eyebrow ridges, the low, narrow forehead, and the excessive bony growth on the occiput or the occipital torus. He compared the skullcap with Neolithic and Roman remains that had been unearthed in Europe and concluded that, although in its particular characters it surpassed any specimen then known, generalized resemblances were so great that the Neanderthal man must be considered human and normal. He reasoned:

> Sufficient grounds exist for the assumption that man coexisted with the animals found in the *diluvium;* and many a barbarous race may, before all historical time, have disappeared, together with the animals of the ancient world, whilst the races whose organization is improved have continued the genus. . . .
>
> There is no reason whatever for regarding the unusual development of the frontal sinuses in the remarkable skull from the Neanderthal as an individual or pathological deformity; it is unquestionably a typical race character, and is physiologically connected with the uncommon thickness of the outer bones of the skeleton, which exceeds by about one-half the usual proportions.

The article of Schaaffhausen as it was originally published evoked little interest outside Germany. By the time of its translation by George Busk and republication in the *Natural History Review* in 1861, however, Darwin's *Origin of Species* was already two years old, and some members of the scientific world were ready to seize upon any specimen that by virtue of its morphological characters would tend to support the principle of transmutation or evolution. As the advocates of Darwin's doctrine were determined to support it by evidence of more ancient and morphologically more primitive forms of mankind, so were the opponents of the principle of evolution determined to discredit any such evidence. The appearance of the Neanderthal specimen gave both proponents and opponents the opportunity for which they had been waiting. It gave them the opportunity of shifting the argument from the relatively static realm of the lower animals to the stage of man himself. Almost every anatomist and paleontologist of any consequence, and several of no consequence whatsoever, felt constrained to add something to the discussion. A review of the writers on the Neanderthal specimen in the decade after Schaaffhausen's publication appeared reads like a *Who's Who* in the anatomical sciences of the middle nineteenth century. The controversy over the real nature of the skull raged through the pages of the scientific journals and on the rostrums of the scientific meetings for many years, concluding only when new discoveries settled the question.

The arguments ranged themselves between two conflicting extremes: (1) the specimen, despite its peculiarities, was that of a normal human; and (2) the specimen, because of its peculiarities, was that of a pathological individual. Generally the followers of the new doctrine of Darwin championed the former view and the opponents of the evolutionary thesis, the latter.

Huxley, the defender and protector of the shy and unassuming Darwin, after a study of a cast of the skullcap, stated emphatically:

> The Neanderthal skull exhibits the lowest type of human cranium at present known, so far as it presents certain pithecoid characters in a more exaggerated form than any other; but . . . inasmuch as a complete series of gradations can be found, among recent human skulls, between it and the best developed forms, there is no ground for separating its possessor specifically, still less generically, from *Homo sapiens.* At present we have no sufficient warranty for declaring it to be either the type of a distinct race, or a member of an existing one; nor do the anatomical characters of the skull justify any conclusion as to the age to which it belongs.

William King, anatomist at Ireland's Queen's University, however, though agreeing with Huxley that the skull was that of a normal individual, could not agree with its putative affinity to modern man. He saw, rather, a much greater resemblance to the apes, so great in fact that the form deserved a

separate specific designation—*Homo neanderthalensis*, the first recorded use of that term for this form. (King later recanted and stated that the name is not applicable since the fossil is not only specifically but also generically distinct from man.)

King's disagreement with Huxley was a minor one compared with that of the proponents of the pathological view. C. Carter Blake, Honorary Secretary of the Anthropological Society of London, was one of the first to express this view when, writing in the *Geologist* in 1862, he maintained:

> All these characters [pathology of the left arm, barrel type of rib box, etc.] are compatible with the Neanderthal skeleton having belonged to some poor idiot or hermit, who died in the cave where his remains have been found. They are incompatible with the evidences which might be left in a Westphalian bone-cave of the remains of a normal healthy uninjured being of the *Homo sapiens* of Linnaeus.

Ten years later, after finally receiving the opportunity of examining the original specimen, Virchow, the great German anatomist, emphatically reaffirmed the pathological nature of the specimen. The most imaginative of the "idiotic" interpretations, however, was that put forth by the German anatomist Mayer in 1864. Decrying the emphasis of the English disputants on the supraorbital torus, he pointed out that this is not an unusual feature in modern man, that, in fact, he "had a classmate, who had a brilliant talent for theosophic studies, in whom the eyebrows sprang out far over the pious face." His interpretation was that the skeleton represents an individual who was afflicted with rickets, evidenced by the pathology of the left arm, and that this condition inflicted so much pain that from childhood on the forehead was puckered in a perpetual frown, resulting in the large supraorbital torus. Furthermore, he insisted, the bowed femurs indicated a horseman, and the skull was more like that of a Mongol than that of an ape, gorilla, or New Zealander. Assembling these deductions, Mayer concluded that the skeleton was that of a rickety Mongolian Cossack belonging to one of the hordes driven by Russia, through Germany, into France in 1814. If not meeting the specifications of scientific objectivity, Mayer's explanation was at least a good story.

And so the argument raged until with the discovery of other specimens with the same physical peculiarities, particularly the two skeletons found in the Spy Caverns of Belgium in 1886, directly associated with glacial fauna, both the antiquity and distinctness of Neanderthal man were settled to virtually everyone's satisfaction.

As for the Gibraltar skull, which not until 1906 was definitely recognized as a Neanderthaloid, it made one brief appearance at the meeting of the British Association at Bath in 1864, after which it was consigned to almost half a century of anonymity in the Museum of the Royal College of Surgeons.

George Busk reported in the *Reader* newspaper of July 23, 1864, that he had received the skull from Captain Brome, of Gibraltar. He noted resemblances to the Neanderthal skull and prepared, with Hugh Falconer, an English paleontologist, to display the cranium before the British Association. Falconer was adamant in his opinion that the skull was human, writing to a relative:

> If you hear any remarks made, you may say from me, that I do not regard this priscan pithecoid man as the "missing link," so to speak. It is a case of a very low type of humanity—very low and savage, and of extreme antiquity—but still man, and not a half way step between man and monkey.

He recognized, however, its distinctness from modern man and in a letter to Busk regarding the display of the specimen suggested the name *Homo Calpicus*, from "Calpe," the ancient name for Gibraltar. The skull was duly displayed, and Busk made a few remarks concerning it. There seemed, however, little tendency to link the form to the controversial Neanderthal skull.

The fate of the Gibraltar discovery and the controversy surrounding its more widely known fossil colleague from the Neanderthal, though small episodes in only one current of the science of a century ago, represent a recurrent phenomenon—and problem—in the development of science. The pat answers of Cuvier and the religio-scientific dogmas surrounding the search for man's origins are almost forgotten in our own enlightenment. But it is sometimes disconcerting to find the Cuviers, the Mayers, and the Virchows—all as equally devoted to the methods and objectives of modern science as any of today's scientists—with intellects imprisoned and imaginations shackled by hypotheses of their own making, hypotheses purportedly based on fact and uninfluenced by metaphysical considerations. It is disconcerting to realize that as their intellects were shaped and limited by the dogmas—often scientific—of their day, so may the intellect of the modern investigator be shaped by the a priori judgments of his time, the unproved hypotheses and overgeneralizations, the result either of the nonscientific environment in which he lives and works or of the sometimes equally nonscientific traditions he follows. One sometimes wonders how many "rickety Mongolian Cossacks" exist in the controversies and conclusions, the debates and deductions, of today.

SCIENCE ON THE MARCH

EFFECT OF 2,4-D ON YIELDS OF OLEORESIN FROM SLASH AND LONGLEAF PINES

FOR over three hundred years the standard practice in the gum naval stores industry in the United States has been to cut wounds on pine trees at frequent intervals—usually weekly—to maintain a flow of oleoresin. Recent work with chemical reagents, however, has demonstrated that the flow of oleoresin from one wound can be increased and prolonged by treatment with suitable chemicals. In 1933, Solodki and Vasskouskaya[1] first reported increased yields of oleoresin resulting from the application of various chemicals to wounds on Scots Pine (*Pinus sylvestris* L.) in Russia. Later work by the Forest Service, U. S. Department of Agriculture, in Florida,[2,3,4] resulted in practical methods of treating wounds on slash pine (*P. caribaea* Mor.) using a 40 percent solution of sulfuric acid and on longleaf pine (*P. palustris* Mill.) using a 60 percent solution.

The sulfuric acid treatment has been used to advantage on commercial operations in two different ways. The treatment can be used at *weekly* intervals for one or two years, with an increase of about two thirds in the total yield of oleoresin from a given number of trees. The treatment also can be used at *biweekly* intervals on the same number of trees for at least five years, with resulting yields equal to those obtained with weekly wounding and no chemical treatment. The biweekly schedule for

A B

Fig. 1. Punch-wound method used in experimental work for screening various chemical solutions for their effectiveness in increasing and prolonging gum flow from Southern pine trees. Final tests of promising chemicals are made using the new commercial method of chipping shown in Figure 3. *A*, close-up of punch wound; *B*, tree with 24 punch wounds.

wounding and treating makes possible an increase of about 60 percent in production of oleoresin per man-day of wounding or "chipping" labor over that obtained by weekly chipping without treatment.

Although sulfuric acid is the best of the chemicals that have been fully tested for naval stores use, it has the disadvantage of being corrosive. Numerous chemicals have been tested in the search for a noncorrosive and harmless substance to replace sulfuric acid. One of the more promising is 2,4 dichlorophenoxyacetic acid.[5]

In a preliminary test made in 1947 on slash pine in northern Florida, several commercial formulations of 2,4-D were compared with respect to their ability to increase the flow of oleoresin when applied to fresh wounds. The procedure used in this test[6] was to make a series of 16 untreated, cambium-depth, circular wounds 0.75 inch in diameter in the bark of each of 4 trees. Oleoresin was collected and measured volumetrically in glass vials suspended on the tree below each wound. Wounds were divided into four yield groups of 16 wounds each on the basis of oleoresin yields from these untreated woundings. The 16 treatments as listed in Table 1 were assigned at random to the wound locations within each yield group. Each treatment was replicated four times and the check sixteen times. Rewounding and treating was done at biweekly intervals. Treatment consisted of wetting the wound completely with a solution applied as a spray and allowing the excess to drain off. Results in Table 1 show that the 8 percent solution of 2,4-D applied as the morpholine salt was the best treatment in this comparison. The punch-wound technique used in this test greatly magnifies the differences in yields between treated and untreated wounds (Figs. 1, 2, 3), and hence the results are not comparable to those that would be obtained on commercial-sized wounds.

TABLE 1

MEAN YIELDS OF OLEORESIN FROM PUNCH WOUNDS ON SLASH PINE REWOUNDED AND TREATED WITH A FORMULATION OF 2,4-D AT BIWEEKLY INTERVALS FOR 6 WEEKS

2,4-D FORMULATION	CONCENTRATION (PERCENTAGE OF FREE 2,4-D ACID)	MEAN YIELDS[1] FOR THREE 2-WEEK PERIODS	
		ml	Percentage of Check
Morpholine salt ..	16	39.9	603
	8	39.9	603
	4	27.3	412
Sodium salt	4	26.3	398
	2	22.2	335
	1	15.4	233
Methyl ester	16	17.2	259
	8	21.2	321
	4	12.5	189
Amyl ester	16	18.7	283
	8	14.5	219
	4	13.4	203
Check	0	6.6	100

[1] Least significant difference between treatments: 5.8 ml at 5% level.

A second test was made on commercial-sized wounds, or streaks, to compare yields of oleoresin from the best treatments of the previous test with yields from the biweekly sulfuric acid treatment and from the conventional untreated streaks.

TABLE 2

MEAN BIWEEKLY YIELDS OF OLEORESIN FROM COMMERCIAL-SIZED WOUNDS ON SLASH PINE AND LONGLEAF PINE

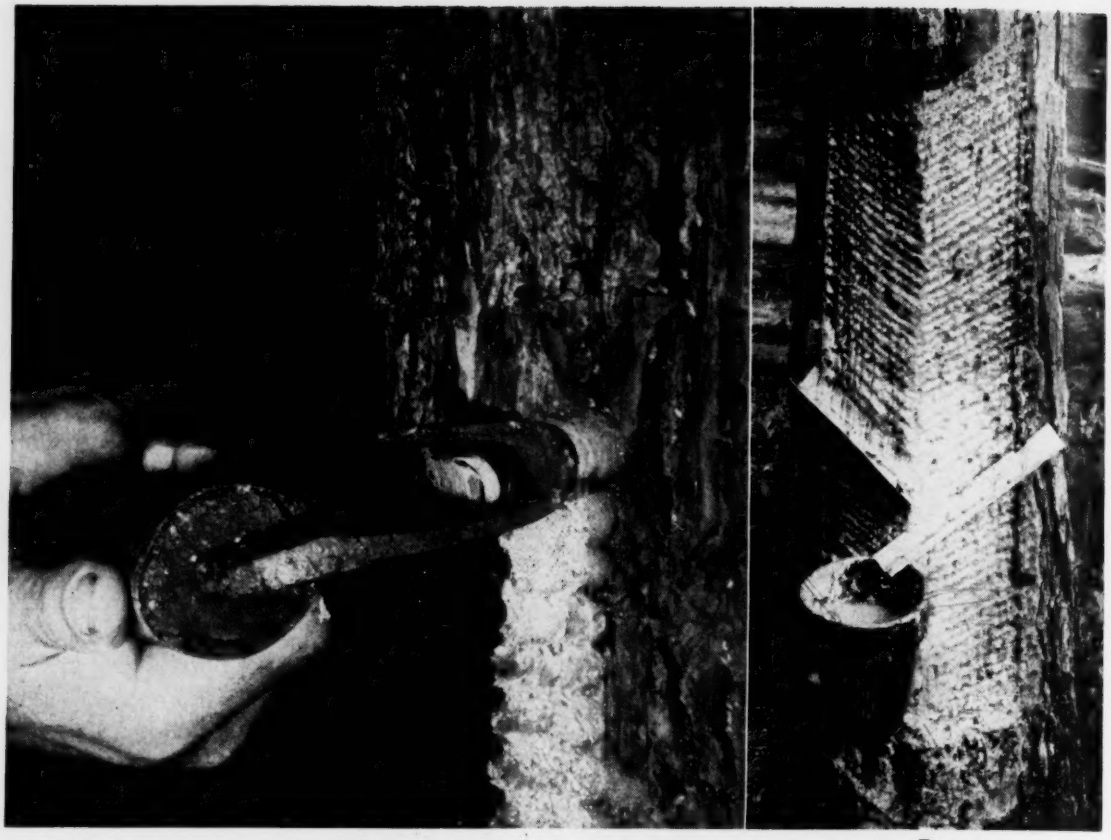
TREATMENT[1]	MEAN BIWEEKLY YIELD PER TREE[2]			
	Slash Pine		Longleaf Pine	
	gm	Percentage of Check	gm	Percentage of Check
Check	190	100	151	100
40% H_2SO_4	168	88	...	...
60% H_2SO_4	...	..	236	151
8% 2,4-D as morpholine salt	187	98	161	107
4% 2,4-D as morpholine salt	136	72	130	86

[1] Check trees were wounded at *weekly* intervals. All others were wounded and treated at *biweekly* intervals.

[2] Slash pine yields are means from four 2-week periods, and longleaf pine yields are means from three 2-week periods. Least significant differences are: slash pine, 8.0 gm at 5% level; longleaf pine, 14.6 gm at 5% level.

Trees were divided into 16 yield groups of 4 trees each on the basis of oleoresin yields from untreated, cambium-depth streaks, 0.5 inch high, and equal in width to about one third the circumference of the tree. Treatments listed in Table 2 were randomized within each yield group.

Results in Table 2 show that on slash pine the 8 percent solution of 2,4-D applied to four successive streaks at biweekly intervals resulted in a yield of oleoresin 10 percent greater than a corresponding number of sulfuric acid treatments. On longleaf pine, however, yields from three biweekly streaks treated with sulfuric acid were about 40 percent greater than with the 2,4-D treatment. When yields from the biweekly 2,4-D treatment are compared with those from the weekly untreated streaks, no significant differences are observed with either species. Hence, during the test period, one



A B

Fig. 2. The old commercial method of chipping a streak. Streaks are made weekly and extend 1/2 inch high and 1/2 inch radially into the wood. No chemical treatment is used. *A*, close-up of old-style streak; *B*, face of tree after being worked for two years.

A B C

Fig. 3. With the new commercial method, only the bark is removed when chipping a streak, as in *A*. *B*, chemical solutions (plastic spray bottle) sprayed on the streak cause an increase and prolongation of gum flow over that obtained with the old wood-chipping method; *C*, face of a bark-chipped tree is smooth, and the tree is not weakened by cutting into the wood.

streak treated with 2,4-D yielded as much oleoresin as two untreated streaks. This increase in yield with 2,4-D is as great as that usually obtained with sulfuric acid on commercial operations. Inconsistencies in this experiment are that the sulfuric acid treatment on longleaf pine resulted in a yield considerably higher, and on slash pine somewhat lower, than the average yields obtained on commercial operations with this reagent.

Application of an 8 percent solution of 2,4-D as the morpholine salt to cambium-depth wounds on pine trees resulted in a certain amount of killing in the phloem and cambium tissues above the wound. The killing was localized, but the phloem tissues in the vicinity of the cork cambium appeared to be much more susceptible to the lethal effects of 2,4-D than the phloem tissues near the vascular cambium. In the cork cambium, killing extended to a mean height of 60 mm above the last streaks in slash pine and 90 mm in longleaf pine. Killing in the vascular cambium was only 27 mm in slash pine and 34 mm in longleaf pine. The relationship between this killing and the increased gum yields is not yet known.

The research is being continued to determine commercial applicability. A full-season test running 32 weeks will be necessary to show whether or not 2,4-D will be effective with repeated applications, and whether it will seriously damage the trees.

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MODERN CLAY INVESTIGATIONS

CLAY is a universal earth material. Few other geologic products have caused as much annoyance—even disaster—or given as much comfort and pleasure throughout human history as clay in its numerous varieties and products. It is an indispensable raw material in many types of modern industry. Ancient races, also, found it most useful for various arts and crafts; its importance in the preservation of invaluable archeologic records of remote antiquity can scarcely be overemphasized.

To many intelligent and educated people, even in this age of pure and applied science, clay is only a form of common "dirt." They know little or nothing about its usefulness to our modern way of life. Others understand somewhat vaguely that clay is an ingredient of brick, tile, pottery, and chinaware, but very few realize that it has extremely valuable uses in other fields, such as the oil, rubber, paper, and metal industries.

Aside from water, and possibly stone and flint for tools and weapons, clay may have been the geologic resource most widely used by ancient peoples. Bricks, tablets, and vessels made of clay are among the relics of prehistoric tribes and nations. With the advance of modern civilization, involving steadily increasing dependence upon geologic raw materials, clay of various qualities came to be more extensively used. Much of the progress in the use of clay in the ceramic arts was made with only a relatively slight knowledge of its fundamental properties. Empirical testing had long been the rule in attempting to use new clays or in making new uses of familiar clays. It is still important.

Physical and chemical sciences have for some decades been concerned with attempts to discover the microscopic and ultramicroscopic characteristics of raw materials. Such precise and painstaking studies in several laboratories throughout the nation have given us a true knowledge of the real composition and structure of clays and, therefore, of their characteristic properties. We are now beginning to understand why clays "behave" as they do—some in one manner, others in another. These recent investigations have been made with the precision tools of modern microscopic and X-ray research now at the command of investigators of ultrafine materials. Combined with new insights into the applicable principles of modern chemistry and physics, new explorable frontiers have come rapidly into view.

Many scientists, outside the particular fields in which the data are studied or used, are more or less unaware of the march of science in the expanding field of clay research. Some of the discoveries during the past two decades have been revolutionary. These notable advances have been made chiefly through the coordinated work of a relatively few petrologists, mineralogists, chemists, and physicists. Soil scientists have contributed much to present knowledge, as have ceramists. The

scientific and economic importance of these discoveries in the realms of geology, soil science, engineering, and specialized industries has been discussed chiefly in technical periodicals.

The word "clay" once denoted any extremely fine-grained plastic natural material, without giving any clear idea of its composition and structure. "Clay material" has recently been introduced as the general term, inasmuch as certain geologic materials having many of the basic characteristics of "clay" are not now generally considered to be clay in an accurate sense. The newer term includes clay, shale, certain soils, and similar geologic materials. All are composed of very tiny grains of mineral particles which, in the aggregate, have plastic properties when mixed with water. Clay is now known to be not amorphous but composed of crystalline particles. The mineral grains are dominantly hydrous aluminum silicates; some are hydrous magnesium or iron silicates. It was a momentous scientific discovery, made by means of X-ray diffraction analysis, in 1923–24, to determine that clays are composed essentially of crystalline grains. This basic fact not only altered the philosophic consideration of clays in their manifold natural relations but gave new insight into their practical utilization.

This significant step far beyond the previous horizon of known facts, or interpretations based on assumed facts, was soon implemented further and supplemented by the discoveries of a few optical mineralogists and petrographers. These investigators have become indefatigable students of the fundamental composition and structure of varieties of clay, or of distinct clay minerals. The few clay minerals that are now generally recognized have as much significance in the understanding and use of clays as do other minerals that have long been more widely known and studied. Each clay mineral group has been given an appropriate name, such as kaolinite, montmorillonite, and illite. The clays of each group have definite, specific properties, a knowledge of which is essential in understanding soils and in determining engineering and industrial procedures.

The modern method of studying clay usually begins with a determination of the kind of clay minerals that compose it by using X-ray, thermal, or optical microscopic methods. Sometimes one of these methods is adequate, but numerous clays are so complex that all methods are required to identify their components. Following this procedure, the character of the nonclay mineral components, including water-soluble salts and exchangeable bases, is determined. Finally, the particle size of the individual mineral grains is studied. All these data permit the investigator to determine the reason why a given clay possesses its own peculiar properties: for example, why one clay is resistant to heat, whereas another is not; why one clay is satisfactory for compounding with rubber, and another is not, and so on.

The major objective of modern clay investigations is to determine the factors controlling the properties of clays. This is of great value in selecting the best clays for particular industrial uses and in devising the best and most economical means of processing clays in manufacturing operations. In some instances such knowledge is of paramount importance, as is shown by this illustration: engineers frequently find it necessary to build a structure on foundation material that may be largely clay whose properties are very detrimental. The clay, for example, may be too plastic, and may tend to flow under the weight of the structure that is to be put on it. If the engineer understands the causes of these detrimental properties, he may be able to treat the clay so that the characteristics are changed. The precise investigation of raw clays for diverse and manifold uses in modern industry is a long story in itself.

Investigations in soil science have shown also that many of the properties of soils, including physical characteristics and the availability of plant nutrients, are intimately related to the properties of the constituent clay minerals. Soils consist essentially of minute crystalline particles of one or more clay minerals plus variable amounts of quartz and other nonclay minerals of sand or silt size. Permeability, water adsorption, plasticity, and compaction are related to the clay constituents of soils. Recently the outstanding discovery has been made that the exchangeable bases in soils have an important control upon the physical properties. In brief, whether the base is sodium or calcium is significant. The essentiality of water absorbed by clay particles is obvious. The water hull that surrounds the tiny clay flakes also plays an important part.

The geologic setting of any soil, especially in terms of the soil profile developed on surficial materials of widely diverse geologic origins, is of critical importance, both for soils as nourishers of plants and as engineering materials. From the standpoint of plant growth, clay controls the general physical and chemical properties of soils, as well as their tilth, acidity, moisture, and supply of water and bases.

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BOOK REVIEWS

PLANT SCIENCE

Botanik der Gegenwart und Vorzeit in Culturhistorische Entwickelung. Karl F. W. Jessen (1864). xx + 495 pp. $6.00. Chronica Botanica. Waltham, Mass.

JESSEN dedicated his *Botanik der Gegenwart und Vorzeit* to two men: G. Gervinus, student of the evolution of mankind; and Ernst Meyer, student of the history of botany. In his preface Jessen pays high tribute to Gervinus as the man who first gave him an insight into the meaning of history as a record of human achievement and of the factors affecting the evolution of the human race; and to Meyer as the man who so successfully applied the methods of the research historian to the field of plant science: a historian with the insight of the biologist and a biologist with the insight of the historian.

The book is the first in a series of offset reprints of out-of-print and classic scientific works, edited by Frans Verdoorn and published by the Chronica Botanica Company. Although written in very readable German, it is to be hoped that it may eventually be translated into English in order to make it easily comprehensible to all students and teachers of the plant sciences in English-speaking countries. In fact, so valuable and stimulating is it that one might wish that it be translated into other languages also. But all serious students of the plant sciences will welcome this replica of the original German, and no botanical library will be complete without it.

It is difficult to review Jessen's book because the reader is likely to become so deeply engrossed in it that he forgets to be critical. There probably will be general agreement with Verdoorn's statement, opposite the title page of the reprint, that "From the point of view of the historian of science, Jessen's book is unique for its cultural approach." Dr. Verdoorn writes also: "I hope that the result of his labors embodied in this reprint, the first volume of our new serial *Pallas*, will be useful and stimulating, not only to the few professional biological historians but to many biologists with historical and humanistic interests." This hope will certainly be realized by all those readers who have a deep interest in plant science and its evolution. The book really is unique in the richness of the cultural background: it is not merely a history of botany; it is a part of history itself. And yet it is a very useful reference book also, thanks to the detailed table of contents and the index.

The period covered is from earliest times to about 1860, the preface to the original being dated November 15, 1863. The book is divided into twenty chapters, including one of about 9 pages dealing with plant knowledge of earliest antiquity, one of about 45 pages on Greece, a shorter chapter on Rome, one on the Eastern Christian world, and another on the Western Christian world. There follow chapters on the Arabs; The Beginnings of Western Science; Universities in the West; The Founding of Western Natural Science; Popular Cultural Development; Special Botanical Studies after Dioscorides; The Beginnings of Systematic Botany under the Influence of the Classicists; The Reformation of Natural Science; The Discoveries of the Seventeenth Century; Agriculture and Horticulture into the Eighteenth Century; Taxonomy from Jung to Linné; followed by chapters on Taxonomy under the influence of Linné and Jussieu; Anatomy and Physiology in the Eighteenth Century; Nature Study and Botany in the Eighteenth and Nineteenth Centuries; and a final chapter on Scientific Botany in the Nineteenth Century.

It is evident from these subjects that Jessen concerned himself not only with esoteric botany but also with the part that knowledge of plants played in the lives of peoples, intellectually, economically, and esthetically. This history of botany is not only a story of the evolution of plant science but also of plant utilization and appreciation; it is a record and evaluation of the impact of civilizations on the development of plant science and of the vital part that plants have played in the evolution of civilization; it is humanized and humanizing history.

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RELIGIOUS EVOLUTION

The Heathens. William Howells. 306 pp. Illus. $3.75. Doubleday. New York.

RELIGION "is an expansion of the whole world of reality . . . an imaginative improvement . . . a set of earnest policies which a group of people adopts under an unconscious compulsion in order to tidy up their distraught relationships with one another and with the universe as they perceive it" (pp. 22–23). To describe and analyze this universal phenomenon among nonliterate peoples, the author has selected an array of topics, including mana and tabu, magic, divination, medicine and witchcraft, life after death, shamanism, and totemism, and has drawn his data from all five continents. Throughout the three hundred pages the author has written with authority, lucidity, and a handsome balance between details and perspective.

Some critical comments can always be made on any work, especially one that is popular and wide in scope. In the present instance, one may, for example, ob-

serve (1) that the author's division between primitive and civilized religions is not particularly valid (from the title of the book, the reader may easily misunderstand that all non-Christian religions are primitive); (2) that the four characteristic traits attributed to all "great faiths" (p. 5) chiefly apply to Christianity and Mohammedanism; (3) that although elsewhere cursory intrusion into Chinese beliefs is common, in the treatment of ancestor worship no reference is made to China, where the cult has been universal and is different in character from that found in Africa; (4) that little attention has been given to the mingled state of science (that is, "the understanding of the correct explanation or principle for anything at all" [p. 19]) and religion or magic (that is, belief or practice which is an "imaginative" expansion of the whole world of reality" [p. 23]) in human behavior not only among nonliterate but among all peoples; and (5) that the author's conclusion that religion cannot be classified because there is no boundary to human imagination (pp. 23, 215–16) would seem to be due to misunderstanding of the reality of the human mind, which is everywhere shaped by cultures.

These and a few other academic questions do not, however, impair in the least the great value of the present volume, which will prove to be fascinating and instructive to all readers. Professor Howells' book must rate among the best popular treatises on the subject.

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SOUND MIND IN SOUND BODY

Mental Health in Modern Society. Thomas A. C. Rennie and Luther E. Woodward. 431 pp. $4.00. Commonwealth Fund. New York.

THE authors outline the purpose of this book as an attempt to add up wartime experiences with mental health, to take a look at the needs and treatments in the postwar world, to consider what education, planning, and reorganization are needed to provide proper and effective treatment to those who need it, and to build positive mental health. Both authors have had wide experience in the mental hygiene field, as well as in mental rehabilitation work, for the National Committee for Mental Hygiene.

The book, which is divided into three major divisions, is excellently edited. In Part One, entitled Lessons From the War Period, the authors discuss such subjects as mental health services in the armed forces, emergency measures for aiding veteran-civilian adjustment, the psychiatric disabilities of war and principles, methods, and results of treatment. This section is well documented with references.

In Part Two, postemergency problems in mental health, including the psychiatric residuals of war, are interestingly though briefly discussed.

In Part Three the authors attempt to integrate the various sources of help in treatment and prevention of mental illness. The contributions of the practicing physician, the social worker, the clinical psychologist, and the minister are ably presented. There follow a discussion of mental hygiene in industry, practical considerations in interviewing and counseling, mental hygiene of family living, and mental health in education. In the final chapter the authors give a summary and forecast entitled The Individual and Society, in which is developed a broad plan of building for positive mental health in modern society.

This Commonwealth Fund book will appeal not only to all workers in the mental hygiene field but to informed laymen who recognize the importance of an intelligent and positive approach to health and happiness.

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NATURE'S CROONERS

Voices of the Night. Recorded by the Albert R. Brand Bird Song Foundation. 4 vinylite records, 8 sides. $6.50. Comstock Pub. Ithaca, N. Y.

THE veteran herpetologist will be startled to hear the most familiar of the frog voices without being wet to the knees in a swamp or pond and searching for the musician in the circle of light from his headlight; he may even regret the degeneration of the times in which his younger colleagues may, with their feet dry, hear the sound medley of the swamp. We may be certain, however, that the remarkably fine album of frog voices of Eastern North America, now made available through the efforts of the Albert R. Brand Bird Song Foundation, of Cornell University, will in the long run stimulate more interest in the nocturnal world of the swamps and shores than all the accounts of frog hunts from the older generation.

The album consists of recordings of the voices of seven species of tree frogs (*Hyla*), four toads (*Bufo*), one spadefoot (*Scaphiopus*), one cricket frog (*Acris*), three swamp tree frogs (*Pseudacris*), and seven true frogs (*Rana*)—twenty-six species and subspecies in all. The reproduction of the voices of these forms is almost startlingly exact. It is only necessary to play them in the dark to recall field experiences to match them. The frog voices in the background, occasional bird voices, and other night sounds give particularly pleasing effects.

The voices of frogs have biological significance as examples of a "species character" of an essentially psychological nature that is frequently far more distinctive and more readily recognizable than any morphological characteristic. A few instances are known, also, of populations of well-known species in which the voice is distinctly different from the norm of the species, as has been reported for the common tree toad (*Hyla versicolor*) of the middle-Atlantic states. There are indications, also, in the

little Solomon Island frog *Batrachyla* that there are distinct differences in voice from island to island, though the frogs themselves are otherwise quite indistinguishable. These matters bear on questions of speciation, since it has often been objected that incipient species differences would be too small to make natural selection effective. That the modifiability of voice without necessity of initial structural change affords an isolating mechanism of extraordinary significance seems to be attested vocally by the frog species of the world. Evolution in other physiological and structural respects may follow in due course.

These records should be in every school, and above all in the home of every naturalist, from nature lover or camper to biologist. There should be an immediate demand for a supplementary album of the distinctive voices of the frogs and toads of the West and Southwest, another of the voices of additional species of the South. We might then hope for samples of the nocturnal chorus of the tropics, which affords so immediate a challenge to every visiting naturalist. A most desirable addition to the series would be a set of nocturnal and diurnal insect sounds.

Over a period of some twenty years, when chance has condemned me to attend a class B motion picture in which the perfectly manicured and marcelled blond heroine was subjected to the terrors of the jungle, I have noted with some amusement that the jungle sound effects, whether intended for Malaya or Africa, have been the same—apparently even the very same record—instantly recognizable as the voice and chorus of the swamp tree frog of temperate North America, perhaps the most familiar frog voice in our continent. The motion-picture industry may now be referred to the Albert R. Brand Song Foundation for a new set of jungle sound records.

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BIOLOGY AND ETHICS

Life and Morals. S. J. Holmes. x + 232 pp. $3.00. Macmillan. New York.

IN THIS stimulating little volume Dr. Holmes, professor emeritus of zoology at the University of California, has set himself the task of revaluing the ethical content of Western culture in the light of the findings of the biological sciences. The enterprise is as formidably difficult as it is urgently necessary. Professor Holmes has made a provocative contribution toward a naturalistic morality as distinguished from value systems based on supernatural revelations and sanctions. His thesis is that ethical standards in our time must proceed not from the concept of man as a divine creation, but from that of man as a living organism and as a member of a society of living organisms, all of whom have the duty of preserving and perpetuating the species as their highest obligation. Professor Holmes' orientation is well put in a passage from his preface:

> Moral conduct in human society is viewed as having much the same functional significance as the cooperative behavior of the parts of the body in maintaining the life of the whole. In the adjustment of their egoistic and altruistic activities, these parts have to do quite precisely the right things, else they and their associates will quickly come to grief. Much the same relations obtain in a hive of bees and to a less degree in other social groups. The life of a human society also depends on the proper conduct of individuals in their several roles; at least, the behavior of individuals cannot be too antisocial or chaos, if not destruction, will soon result.

For all scientists—physical, biological, and social—this book is well worth reading. It is full of new insights and suggestive reformulations of age-old issues. It suffers, however, from a major defect. Dr. Holmes has not taken cognizance of the contributions of Sigmund Freud and his successors to an understanding of the structure and dynamics of human personality. An essay on ethics in pre-Freudian terms, even from the pen of an able biologist, is almost as quaint as an essay on biology in pre-Darwinian terms or an essay on economics in pre-Keynesian terms. This "blind spot" is stark and startling in these pages. It vitiates much of the value of what is here said, although the residue is still worth serious attention. It is to be hoped that Professor Holmes may someday find time and occasion to amplify and perfect his trenchant argument in the light of contemporary psychoanalysis and psychiatry.

FREDERICK L. SCHUMAN
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Ethics for the Atomic Age. Ana María O'Neill. 411 pp. $3.00. Meador Pub. Boston.

UNDER the shattering impact of events unleashed by the discovery and artificial liberation of atomic forces, scientists asked themselves whether a changed physical universe does not require a total reorientation in our moral universe. In a lively and readable book, the author tries to answer this perplexing question and thus to give a blueprint for an ethical society for our time. In a penetrating analysis of human nature she resolves the complex personality of man into an affinity for things and an affinity for correct solutions. Inasmuch as he, as a physical being, cannot escape the physical and biological laws of nature, as an intellectual entity he cannot disengage himself from the urge to find correct solutions for problems caused by the impact of his physical environment or contact with other human beings. In the most elucidating part of the work, "The Voice of Moral Engineers" is recorded in their endeaver to coordinate physical and intellectual urges: through justice, the message of the Greek philosophers; through intelligence, the doctrine of American pragmatism; through duty, the essence of Kant's categorical imperative; through love as preached in the Sermon on the Mount. All these ethical precepts are rooted in the concept of man as an essentially moral being endowed with

inalienable rights that presuppose a moral order above the political and economical organization of society. However, although the author succeeds in analyzing the interaction of physical and intellectual forces which determine our own moral attitude and the character of our society, she does not attempt to find an ethical solution for national conflicts so much more threatening in this atomic age. But it is exactly here where the lack of a unifying principle of sufficient moral power has become ever more apparent. Inasmuch as man will never reconcile himself to the coexistence of two worlds in a political sense, so he will never be satisfied with the concept of the separation of his universe into a material and a spiritual world. For his reason tells him there can exist but "One World," and he will never cease to integrate his scientific and moral experience into a physical and at the same time an ethical universe.

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THOSE CLEVER PEOPLE

Farmers of 40 Centuries. (2nd ed.) F. H. King. 379 pp. $5.00. Rodale Press. Emmaus, Pa.

THIS book, published first in 1911, contains the observations of Dr. King during a trip of several months made for the purpose of studying agricultural practices in the Orient. Dr. King was an eminent agricultural scientist and a well-qualified observer. He presents a thorough account of the cropping practices, crop rotations, drainage and irrigation methods, and soil management procedures followed in parts of China and, to a lesser extent, in Korea and Japan. There are, in addition, numerous observations on living conditions, housing, and other phases of life in these countries.

The central theme of the book is the painstaking care of the Oriental farmers to maintain or enhance the fertility of their soils by saving and returning to the soil everything that might serve for that purpose. This includes farm manures, straw, ashes from the fires, soil dredged from ditches and canals, grass and other vegetation harvested in the hills, and even night soil from the cities. Dr. King stresses also the density of the population, limitations on arable land, and the need for producing more and more food. Now, almost forty years after the first edition of this book, these same practices are followed and the same problems remain. Agriculturalists who have been in Japan in recent years will be struck by the similarity of conditions today with those described by Dr. King nearly four decades ago.

The text is illustrated by 209 figures—photographs of scenes in China, Japan, and Korea. Some of the pictures are excellent. It is unfortunate that the editor or publishers did not use some selection, for many are too blurred to be useful.

This book should be a valuable reference for agriculturalists seeking knowledge of conditions in the Orient. It is doubtful if it will find a wide audience among lay readers. The style is ponderous in places, and there are too many statistics on production, fertilizer application, and monetary returns per acre. One might wish also that the book had been dated. There is no date of this edition and no reference, except on the dust jacket, to the fact that this is a second edition of a book published in 1911. Statistics on population, prices, acreages, production, etc. are for the early years of the twentieth century and do not apply today. The casual reader might easily be misled into considering them currently valid.

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PRACTICAL AESTHETICS

The Unfolding of Artistic Activity. Henry Schaefer-Simmern. 201 pp. Illus. $5.00. Univ. of California Press. Berkeley and Los Angeles.

IN SO FAR as aesthetics is a legitimate branch of inquiry, it seeks the answers to such questions as: What are the psychological processes behind artistic production? What are the sociological factors conditioning taste? What is the cultural function of art forms in a given culture? This is not to say that writers in aesthetics must themselves be psychologists or anthropologists, any more than a philosopher of science need be a creative physicist. It is only to say that the task of aesthetics is similar to the task of the philosophy of science: the philosophy of science examines the writings of scientists to separate empirical and analytical elements, to arrive at what Carnap has called the rational reconstruction of science. Similarly, the philosophy of art has the assignment of examining the language of art for the purpose of determining which sentences of criticism are empirical, that is, derived from psychology and the social sciences; which statements are analytic, that is, definitions, derivations, and the like; and which statements are pragmatic, that is, requirements of purpose and intention. In short, the philosophy of art should attempt a reconstruction for the language of criticism.

It should do so. All this, then, is programmatic for aesthetics. This program is not well understood by some writers in this field (just as the program for the philosophy of science is not well understood by some philosophers), and this confusion leads to the disappointment of those who appeal to aesthetics for a rational interpretation of art.

This explanation makes it clear why Professor Schaefer-Simmern's book deserves the notice of scientists, philosophers, and all those who may have looked in vain to aesthetics for information of the fundamental sort about the artistic process. We wish to know from the aesthetical philosopher, for example, which alleged principles of form and "composition" are programmatic—whether "traditional" (program adopted from the art of the past) or "modern" (program urged by artists in the present)—and which

alleged principles have a psychological function in what Schaefer-Simmern calls "visual conceiving," that is, a process growing out of the actual making of a work of art.

If there is such a psychological function of artistic activity, as apart from the conventions of representation of making surface structures that are only externally coherent, then it should be determinable by experiment and observation. Schaefer-Simmern describes four such experiments with untrained groups: one with mentally defective girls and boys, one with delinquent boys, one with refugees, and one with business and professional men and women. The process of learning to make a work of art is a very complicated one, and the experiments reflect this complication: we are shown, in actual visual facts, the problems met and solved by the would-be artists, from the first scribbled drawings to (months and years later) finished works of art on a high level of expression. The stages in these solutions are clearly distinguished and fully described. Each problem presents the novice artist with the need of finding his own solution in terms of the job actually before him, and the evidence seems clear that he finds it in his own stage of development in visual conceiving. That these stages are similar in respect to similar problems for all the persons involved is proved. Moreover, Schaefer-Simmern suggests that they are similar for all artists in all times and cultures—a most important law resulting from his thesis that these processes are psychologically determined by the problems themselves. It is this part of *The Unfolding of Artistic Activity* that is somewhat sketchy; but a man cannot say everything at once, and we can hope for a later definitive study from the author to amplify this part of his demonstration.

The demonstration in the present study is limited to a detailed description of the four experiments mentioned. Without such experimental data no amount of comparative aesthetics would be conclusive. If those in art academies who have a vested interest in teaching "composition" will suspect that Schaefer-Simmern has got out of his experiment a sort of common denominator primitivism because he instilled it there in the form of subtle precept to members of his groups, these critics have only to repeat the experiment and find out for themselves. That is the advantage of scientific work: it is self-correcting. Incidentally, the educational and other social advantages of duplicating this experiment on a wide scale seem so promising as to make one wish for the greatest possible exploitation of all opportunities. As John Dewey says, in his foreword to this book, the demonstration here "provides the pattern and model of the full and free growth of personality and of full life activity, wherever it occurs, bringing refreshment and, when needed, restoration."

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THE WORLD WE LIVE IN

Understanding Our Environment. Franklin B. Carroll. vii + 313 pp. Illus. Winston. Philadelphia.

Understanding Our World. Franklin B. Carroll. vii + 412 pp. Illus. Winston. Philadelphia.

Understanding Our Universe. Franklin B. Carroll. vii + 313 pp. Illus. Winston. Philadelphia.

IN THESE new, vivid books prepared for the seventh, eighth, and ninth grades, the principles and methods of science are thoroughly and fascinatingly described. The author has realized the problems connected with the preparation of such a series and has made an exceptionally successful presentation. The need for outstanding science instruction in the junior high school is very great, for it is at this time that the student must form a critical, "scientific" attitude in regard to the problems of his environment.

The science text has three main functions. Two of them are to stimulate the student's interest and participation in science and to impart in a flexible mind the merits and methods of scientific thinking. A scientific attitude is not a strange, impractical abstraction; it is merely logical thinking—something which can well be applied in many fields other than that of science. The science text should emphasize the practical, everyday aspect of "scientific method." The third function of the science text is to furnish a broad, yet concrete, background in the biological and physical sciences and to relate these facts to the panorama of modern life. These fundamentals can be presented only in a form familiar to the student. Abstract principles alone are not proper subjects for a junior high course. The ABCs of modern science, therefore, must be developed from things the student knows of and is curious about. The radio, the airplane, the flower, the skyscraper—these are the things that students think about. All of them must be used to develop the abstractions of scientific theory, principles which are, of course, quite necessary for advanced science instruction.

The "Interpreting Science" series seems to be above average on all three counts. The approach to subject matter is clear and practical. The text is filled with stimulating examples and illustrations, and students will be attracted by the numerous clear diagrams and pictures. Black-and-white and color photography are put to good use in promoting interest and clarifying subject matter.

From the first chapter to the last, the series emphasizes the methods we call "scientific thinking." The development of a scientific attitude is prompted in the student with the aid of many examples from everyday life. Thought-provoking questions at the end of each chapter provide opportunity for putting scientific thinking into practice. The ninth-grade text, especially, is highly informative, and the presentation of subjects is well planned. Above all, the books develop scientific method as a useful attitude. Says the author:

The method of science is kept constantly before the

pupil's attention. Establishment of facts through observation is presented in experimental studies as well as through text and narrative. The organization of facts into a body of knowledge, their interpretation in a generalization at the pupil's level, the use of such generalizations in the interpretation of facts in a new situation, the framing and testing of hypotheses, receive attention at strategic positions. Solution of problems by scientists illustrates the method in use, and guidance in solution of problems provides opportunity for its exercise.

In view of the merits of this new series, it seems correct to appraise these books as outstanding examples of modern scientific texts.

ANDREW KENDE

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NONTECHNICAL SURVEY

Handbook of Psychiatry. Winfred Overholser and Winifred V. Richmond. xii + 252 pp. $4.00. Lippincott. Philadelphia.

THE product of the collaborative efforts of a psychiatrist and a clinical psychologist, this volume presents a nontechnical survey of the various types of mental disease, their causes, symptoms, and prognosis.

The mental diseases discussed include the psychoses (the so-called insanities) and the relation of these severe behavior deviations to such organic factors as paresis, encephalitis, infantile paralysis, essential hypertension, brain tumors and injuries, alcohol, drugs, and the organic changes accompanying middle and old age. The functional psychoses (those without detectable organic damage) are then considered, followed by a discussion of the psychoneuroses—anxiety states, neurasthenia, hysteria, and obsessive-compulsive states —and their prevalence and importance in our daily affairs, including delinquency, crime, and war. A special section is devoted to a discussion of psychiatric conditions in children.

Related to the discussion of the symptomatology of each type of mental disorder is a section on causes, with special emphasis on a debunking of popular beliefs concerning the origins of mental disease.

Of greatest interest to the layman, however, will probably be the discussion concerning prognosis for each disorder and the description and evaluation of various psychotherapies. Here again, emphasis is placed on what cannot as well as what can be done, cautioning the lay reader against credulous acceptance of quick cures frequently promised by "psychological" quacks.

Since this volume is intended primarily for the nontechnical reader, it is perhaps unfortunate that the title, through its use of the word "handbook," implies an authoritative, technical summary of the subject matter for the specialist in mental disorders. For the specialist the book offers nothing new in either content or organization; for the layman it provides a sound survey of major behavior deviations. The book might have proved more valuable to the lay reader, however, had it included, besides the psychoses and psychoneuroses, a consideration of the psychological mechanisms employed by normal persons in their struggles with everyday problems.

This volume is one of the Non-Technical Scientific Series of the American Association for the Advancement of Science.

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A MAJOR SOCIAL SCIENCE STUDY BY A BIOLOGIST

Sexual Behavior in the Human Male. Alfred C. Kinsey, Wardell B. Pomeroy, and Clyde E. Martin. xv + 804 pp. Illus. $6.50. Saunders. Philadelphia and London.

UNLIKE most scientific books, the so-called Kinsey report is by now familiar to millions of people. An unusually large number of scholars and laymen alike have bought the book; others have read reviews of it in scientific and popular journals; and many others are reading articles and pamphlets based on it. Before the cycle is completed, we can expect to see many books about the Kinsey report and many public discussions pro and con. It has had a tremendous impact on Americans of all walks of life. Why?

For one thing, it is about sex, and sex is always interesting. But there have been hundreds of books about sex that have received nothing like the attention given this one. There is more involved, therefore, than just the topic. The big difference, in fact, between this book and others in the field is that it tries to tell us much more about sex, and about sex in all sorts of men, than any previous book.

It tells us, for example, that there is great variation in the sexual outlet of male adults and that variant sexual patterns are much more common than has been supposed. It describes the rise and wane of sexual power in the course of a man's life. It gives us new insights into the types of outlet in different cultural, educational, and religious groups.

The discussion of these facts, and of many more like them, in this report, alters radically our conception of what is statistically "normal" in American male sex life. The many graphs and tables in the book bring out other details. Altogether, they paint a rather different picture of male sex life than most clinicians, scientists, and laymen have given us before. In fact, what has often been depicted as abnormal and delinquent sex behavior now seems, according to Kinsey, to be rather normal in the population as a whole.

But no one reads the book without asking the all-important question: Can we believe what we read? Did the Kinsey workers get a fair sample of the American male population, or did they get only the more boastful and exhibitionistic members? Can their interviewing methods be trusted? Are the methods

capable of detecting the deception and outright lying inevitable in such a private matter as sex? On the answers to these questions hinges the real value of the book.

How fair is the sample? That is the first big question. The sample is large: it includes 5,300 cases. They represent all ages (five to ninety years), rural and urban societies, all educational levels (grade school through college), and all kinds of people, including prison inmates, male prostitutes, clerks, businessmen, college professors, and clergymen. If we look closely at the number of people of each kind and class, we see that they are not representative of the American population at large, however. There are too many special classes, such as the professions and prison inmates. There are too many in the Midwestern states and too few in other areas of the country. Is that bad? It probably is not, for Kinsey, well aware of the skews in his sample, consulted the U. S. Census statistics and, in making up his final figures and charts, usually weighted his groups according to their incidence in the general population. That is a fair correction to make and cannot be seriously criticized.

Once satisfied that Kinsey's statistics have been adequately corrected for disproportions in the classes of people studied, one can still ask whether the people he interviewed were representative of the groups from which they came. Did he get too many boasters, too many people preoccupied with their sex problems and only too willing to talk about them? That is a more difficult question to answer. Certainly Kinsey did his best. He tried to secure a fair cross section of people of various types. More than that, as a control against bias in his sample, he ran a good many so-called 100 percent tests; he worked until he had interviewed 100 percent of the members of a fraternity or an institution. When we compare the results of these complete studies with those in which only a small percentage of the group was interviewed, differences are noticeable, though not large. In fact, if we read such comparisons carefully, we must conclude that, if there is any bias toward boasters or unrepresentative individuals, it cannot be great. It cannot, in the main, change the general results very much.

The final question is: Can the interviewing method be trusted? Kinsey went to a great deal of pains to work out his interviewing method and then to put it to every possible test. Certainly the interviewing was comprehensive: it included a battery of 531 items, and it was so constructed that it contained many cross checks against inaccuracy and deceit. It is clear, too, that Kinsey and his interviewers used many clinical devices to guard against false information. Finally, many repeat interviews were given to the same individuals, separated by weeks, months, or years, and the agreement between repeat interviews, given by the same or different interviewers, was practically perfect. It is true that people have imperfect memories of their past behavior, especially when many years have gone by, and it is difficult to assess how much bad memory may have affected the results. When all is said and done, however, Kinsey's interviewing method must be considered fairly complete and the best and most reliable that has ever been used in this field. It is hard to see how the final statistical statement of his results could be very wide off the mark.

To this reviewer, then, the Kinsey report is an outstanding achievement. The methods were basically sound; they led to trustworthy results—more so than the vast majority of biological experiments. If they are in error, it is not by far. And they certainly give us the truest, most accurate picture that we have to date of sex behavior in the American male.

The implications of Kinsey's results for scientists, legislators, physicians, and public officers are tremendous. To the scientist, they make clear that man is a typical mammal, exhibiting the many variations in sexual behavior that we see in other mammals. To the physician, they point a warning not to construe many kinds of behavior as abnormal, nor as unduly important in the patient's mental or physical health. To the legislator, they say clearly that our current laws do not comply with the biological facts or normal sex behavior. And to our judges and public officers, they point the way to a much more intelligent handling of the sex offender and sexual delinquency.

Outstanding as it is, the Kinsey report is not finished. Kinsey and his collaborators have thousands of interviews of female sexual behavior that will appear before too long in another book. And they are at work collecting more data to make a much more careful analysis of the social and biological factors in sexual behavior. Eventually they plan to present a series of volumes on sex behavior. These will certainly modify and extend the conclusions of the first volume. The final result should be one of the most outstanding contributions of social and biological science to the welfare of millions of people in all walks of life.

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TECHNOLOGICAL NOTES

Centralization vs. decentralization. "Paying the freight" to make manufactured products the same price all over the nation seems threatened by the Supreme Court's recent decisions. Social planners may believe the result will be a scattering of factories. Just the opposite may be the case. Uniform prices have benefited the unindustrialized parts of the country. If Mississippi, not a heavy producer of either books or steel, must pay more for them than eastern Pennsylvania, she has small chance of building up her own publishing or metal-fabricating industries. Her people, already poor, must pay still more for the products of the East. Many factors must be considered. As just one example, the extra weight of glass in fluorescent tubes as compared with incandescent electric bulbs has bearing on the dispersion of lamp factories. New plants are going up near local markets. Another reason for their distribution is that they can tap the abundant supply of labor in small towns instead of adding to the congestion of cities.

Electrapane becomes warming. On windshields, various methods have been used to melt ice and dispel fog. One of the newest, used widely in the war and apparently proving useful in peace, is the deposit of a thin film of conductive oxide on the glass. Alternating or direct current bucking the resistance of the coating produces heat. The film is on the inside, next to the plastic filling of safety glass, so the conductor is not exposed. The casting is durable and practically invisible. Reflection is cut down by keeping the film thickness a multiple of one quarter of the wave length of yellow-green light. Libby-Owens-Ford, owner of the invention, calls the glass Electrapane.

Jet helicopters. Research on jet propulsion for helicopters is a new undertaking of the General Electric Company. The idea of jets on the ends of the blades of the helicopter seems novel, but it is a logical extension of the theory.

Fragile vitamins. Quick cooking with "radar" waves and other uses of high-frequency radiation in preparing foods may have some drawbacks. If the rays used in preserving or preparing the food destroy vitamins, the result may be a palatable shadow without much substance. The subject is being investigated at MIT.

Special glass. Big tanks of molten glass, constantly supplied with fresh batches of ingredients, make glass production for many purposes a continuous operation. Not so the special glasses used in electrodes for delicate chemical analyses. That kind of glass-making is entirely different. Leeds & Northrup electrodes take about a pound of glass a month. This important batch is especially compounded and melted in a small electric furnace by L & N workmen.

Tomorrow's train. Announcements of anything as belonging to "tomorrow" are dangerous. The term is exceedingly elastic. "Life in 1960" in *Kiplinger Magazine* relegated 1947's "Train of Tomorrow" to a Georgia branch line. A lighter, sleeker ultra-modern train is suggested by the Chesapeake & Ohio Railway. It would do away with the independence of the individual cars. Each car would really be a trailer, 30 feet shorter than present cars, with one wheel truck at the rear. The low center of gravity, the trailing wheels, and the light weight would make 150 miles an hour possible, the designers think, and even on curves high speed and smooth riding could be obtained. Instead of each car's having its own generators and batteries, all such services would be concentrated next to the locomotive.

Magnesium Magazine. A few years ago the Dow Chemical Company was practically the only producer and promotor of magnesium to lighten metal products. It was made from Michigan brines or from sea water. Then came the war and the widespread use of magnesium alloys and production of the metal from dolomite in Ohio and from brucite at Las Vegas. Not so much magnesium is needed now, but magnesium continues its "march," according to the *Magazine of Magnesium* published by Brooks & Perkins of Detroit. A light mask for the baseball catcher may be of magnesium. Cabinets for delivering bread and cakes weigh an easily handled 68 pounds, against 270 pounds for similar designs in steel.

Strong and light. Crated freight increases shipping costs. Strong crates of "Versalite," a light thermo-setting plastic made by the U. S. Rubber Co., and insulated with "Flotofoam," an expanded plastic that doesn't weigh a pound per cubic foot, are available as freight savers. Because the insulation is so good, quick shipments of frozen foods may dispense with ice, either dry or wet. Longer distances and times require inclusion of ice in the container.

Warm hearth. When the fireplace doesn't draw properly, some experienced fire builders spread a newspaper across the opening, concentrating air at the bottom. That procedure is a trifle dangerous and tiring. Moreover, it doesn't prevent the great loss of heat as warmed air is drawn out of the room, causing chilly drafts. A maximum of cheer and radiated heat, with a minimum of smoking and loss through drafts, is the claim of the makers of a glass fireplace-control screen. The louvres of plate glass can be turned down as desired to permit the fire to be seen and felt and still confine the departing air to a small space at the bottom of the fireplace opening. The makers, Dollinger Corporation, of Rochester, say that the glass screen will make a fireplace damper unnecessary.

Vernier slide rule readings. In the century and a quarter since the slide rule was invented, practically everything, presumably, has been discovered about its graphic application of logarithms. Nevertheless, a Miami University senior recently reported to the Ohio Academy of Science a possibility that seems not to be mentioned in the instructions for using the slide rule. A result on the *D* scale may be read to one more place if the corresponding part of the *C* scale—so adjusted that 10 of its divisions cover 9 of the divisions on the *D* scale—is brought up to the *D*-scale ruling in question. This student made the interesting discovery while he was still in high school. He finds it particularly useful with cheap and poorly graduated slide rules.

Light. Intensity, safety, and coolness are claimed for a miniature fluorescent microscope lamp by Burrell Technical Supply Co., of Pittsburgh. The highest heat is 120°, and delicate specimens are safe without trimming the light.

"The greatest invention since sunlight" is the mysterious slogan on a picture of the portable "Strobo-Lite" of Wilcox Photo Sales, Los Angeles. The output is compared favorably to that of a photoflash bulb. The weight of this two-lamp, battery-operated light source is 70 pounds.

Light on the school budget is a Bausch & Lomb microscope, stated to be standard in practically every way but priced at less than $100.

Thin film. Emulsions only one tenth as thick as a human hair are helpful in autoradiography—the subject "takes its own picture" by effect of its radiation on film. Radiation comes off in all directions, so extra thinness helps "pinpoint" the location; there is little room for travel in the film. Eastman makes it.

Closely related is Eastman's announcement that its emulsions have recorded the "track" of electrons, the streak made in the film as an electron travels about two thousandths of an inch, affecting 6–28 silver grains.

Magnetic clutch and iron powders. Making a smooth connection between a moving motor and running gear is a clutch problem with which every motorist is familiar. A Bureau of Standards development seems to be something new and promising in clutches. The space between the plate that will drive and the plate that will be driven is filled with oil containing a thick suspension of iron powder. When an electrical connection makes that space also a magnetic field, the iron particles line up and hold to each other and to the plates, making a smooth clutching action. A magnetic clutch idea would be worth little without suitable iron powders. Tiny granules of iron, made by General Aniline & Film Corporation for use in radio cores, have proved useful for the new clutch. The granules are made by reaction of carbon-monoxide gas and iron oxide.

Night guide. As many of us who have groped for a bedside flashlight know, the darkest of moments may seem an eternity before the article is found. Life is made a little brighter by availability of a flashlight with a case that, properly exposed to light during the day, glows at night. It is a Winchester product.

As *Paint Progress*, published by the New Jersey Zinc Company, reminds us, luminescence may be a valuable property of many other items in our environment. Faintly glowing guides contributed greatly to safety on blacked-out ships and in darkened towns during the war, and even in peacetime there are many places where luminescent paint may afford protection to more than the surface.

Protective protein. For mildly corrosive atmospheres, the National Bureau of Standards has developed a combination of protein and a chrome salt to protect metals. The two-step process has most possibilities. The article is dipped in a water solution of gelatin, albumin, or other protein and allowed to dry; then there is another dip in chromic acid or in dichromate of zinc, iron, or nickel. The film is slightly yellowish and transparent. It is hard, adheres well, is almost insoluble in water, and will stand heating up to 150° F. It is not as durable as paint, but it has advantages over greases and waxes.

Even on Pikes Peak. Raucous horns from trains on main-line railroads are vivid reminders of what some word coiners probably call "Dieselization." Almost everywhere the steam locomotive, with its panting exhaust and musical whistle, seems on the way out, although the C & O—a great coal hauler—is sponsor of a coal-burning steam-electric engine. Changes of this kind are not easy. Diesel fuel comes from oil, and oil has a much more uncertain future than coal—except that coal will probably be a source of oil in the future, although the expense will be greater. Locomotive factories must redesign and retool. Railroad roundhouses and workmen must be made over to fit the new locomotive. Diesel power has even come to the famous cog railway up Pikes Peak, the National Geographic Society reminds us. Last year two Diesel-electric locomotives took over from the old tilt-boiler puffers, now kept as stand-bys for extra-heavy traffic. The railroad was opened for business in 1891 and has carried more than a million passengers.

Business in building. Residence construction for personal use, known as "building a home," is often undertaken in something of an emotional state. But house construction is a business. Items of contract, and realism in dealing with architects and contractors, are discussed in a new circular of the Small Homes Council of the University of Illinois. This *Business Dealings* is the twentieth in the series of free pamphlets issued by the Council.

M. W.

COMMENTS AND CRITICISMS

WHAT IS TIME?

Hermann Lotze, in his *System der Philosophie* (part 2, 1879, "Metaphysik"), rejects an "abstracting comparison" which leads to the concept of a kind of general laws of nature "which first exist as valid and to which then a world would come submitting itself to them. . . . In this sense, we can find better justification in the often heard sayings according to which not time is the condition for acting [*Wirken*], but acting produces time." This, however, is not "a permanently remaining product, an actively real time," it is a subjective form of intuition.

The "often heard" argument contains one of those Copernican reversions which Kant claimed to have performed by his system and which are quite frequent in the development of science (see, e.g., my article in *Osiris*, 5, 1938). Thomson King ("On Time as a Product of Motion," SM, October 1948) describes as his discovery that "time is the product, not the cause, of events." Time, he says, is not "elemental," not "a primary reality, a something in itself, its definition as a product of motion gives it a reality that is consistent with the universal as we know it. . . ." The author does not explain how we can know motion without bringing to it the concept of time. How can motion produce time? The labors of centuries led to the concept of heat as a product of motion. In 1825, Berzelius still lists as "dynamides"—namely, something like force—substances without matter, heat, together with light, electricity, and magnetism. All four of them were found to be based on motion. Is time the fifth "product"?

In one of his tragedies, Strindberg introduces a young girl who says: "Time is that which flees. I flee; I am time!" King's argument is of about the same consistency.

Does the pendulum while swinging produce time? Is the amount of time increasing with every moving ship, train, plane? The author cannot mean this, although he writes: ". . . the time of any interval, a minute or a century, is the result of the summation, the integral of the motions that took place during the period." He seems to mean the universal Motion, the motion of the "billions of molecules," etc., which in totality are comprised under the laws of energy. Such an extrapolation belongs to the group of generalizations which, according to Ernst Mach and many others, are devoid of meaning when extended to the whole universe.

As a final proof, King lists a series of identities between time and motion without realizing that this means the contrary of his "discovery." The two are connected but not by one producing the other as an object of either "primary" or derived reality. The process does not take place without the human mind. The attempt to omit this intermediary was made long ago, at the "time" of the violent discussions between realists and nominalists in the eleventh and following centuries. Discussions of this rank were extended to the "reality" of forms and their "production" by matter, of the relationship between individuum and community, and finally of microcosmos and macrocosmos. Since then, we may have made progress in distinguishing observations and abstractions, between event and observer. Motion produces change of position and composition, it does not produce time as, say, yeast produces enzymes. Time is the mental, and sentimental, addition we bring into events. That this is done through objectivation and standardization, in short, through comparison, prevents this addition from being either arbitrary or a mere identity with motion.

EDUARD FARBER

Washington, D. C.

The article by Thomson King in the October issue is about as bad as his earlier one, "On Life as a Separate Entity" (February 1947). It would be better to solicit papers of this kind from philosophers rather than scientists —if Mr. King is one. For these are matters of abstraction and thought and not open to scientific study. . . .

Scientists study matters of fact or events, not categories of abstractions. I challenge Mr. King to show me a matter of fact or event called "life" or "time." The absence of clear philosophic thinking in Mr. King's paper emerges from such statements as ". . . time is a product of motion . . ." (p. 290); or ". . . a universe that contains matter, but has never had motion . . ." (p. 291); or ". . . time is the product, not the cause, of events" (p. 292).

The products of motion and events can only be again motion and events, but not time. Time is an aspect of motion and events, just as is space; in other words, an abstraction of our minds to which no real object corresponds. That this is so is proved by the results of Minkowski (whom Mr. King misunderstands in his quotation on page 292) and Einstein, for instance. I wonder whether Mr. King is going to present us with similarly learned papers on such abstractions as "mind," "color," "soul," or "taste." Perhaps he had first better read Plato, who treated of these things much more logically and poetically.

HANS TISCHLER

Roosevelt College of Chicago

NOT MERELY FOUR SQUARE WALLS

Even though there seems to be a vast difference of opinion between us, I thoroughly enjoyed Lindsay Lord's letter in your February issue. I can't accept his cost figures as typical of the industry, however.

Since Mr. Lord has raised the question, I do want to say that I have wandered through Williamsburg and Mount Vernon many times and that I have a very deep admiration for these old houses. My favorite pilgrimage is to Charlottesville and Monticello. Thomas Jefferson! There we have a true nonpareil, a designer whose aesthetic sensibilities are matched by his mechanical ingenuity, a gadgeteer with a genius for the beautiful!

The only drawback to such visits is that I always wind up with the deflating thought that after a century and a half we are still trying to imitate these architectural masterpieces of the past for their surface characteristics—and not doing too good a job at that. We completely miss the point that if these early designers were still alive they would be trying to do now what they were doing then—seeking to do the utmost with the materials at hand. . . .

If I understand Mr. Lord correctly, his standard of perfection in housing must always be at least twenty-five years old. This does not seem to me to be in the best American tradition. Certainly it cannot be called scientific.

C. THEODORE LARSON

Arlington, Virginia

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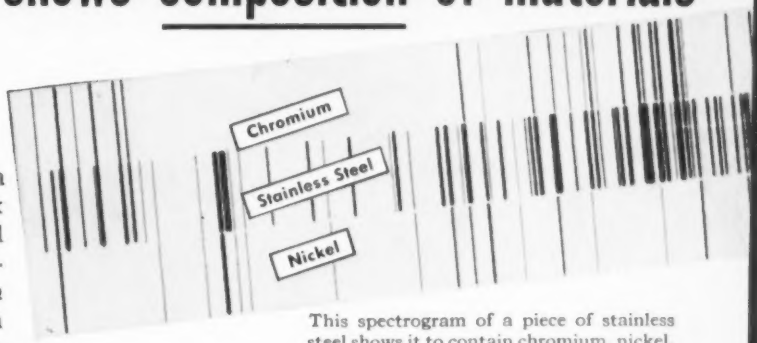
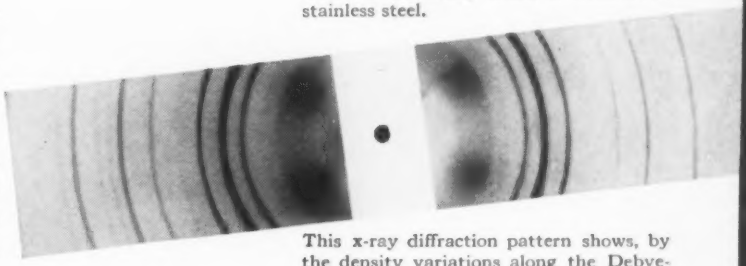
3 ways photography shows composition of materials

Spectrography . . . To record composition quantitatively and qualitatively, Kodak offers 108 specialized materials. For general spectrographic use, however, Kodak recommends two emulsions: *Kodak Spectrum Analysis No. 1 Plates and Films* for high contrast, low background density, and low granularity; and for a more uniform contrast-wavelength relation, *Kodak Spectrum Analysis No. 2 Plates and Films.*



This spectrogram of a piece of stainless steel shows it to contain chromium, nickel, and iron, but none of the other alloying elements commonly found in commercial stainless steel.

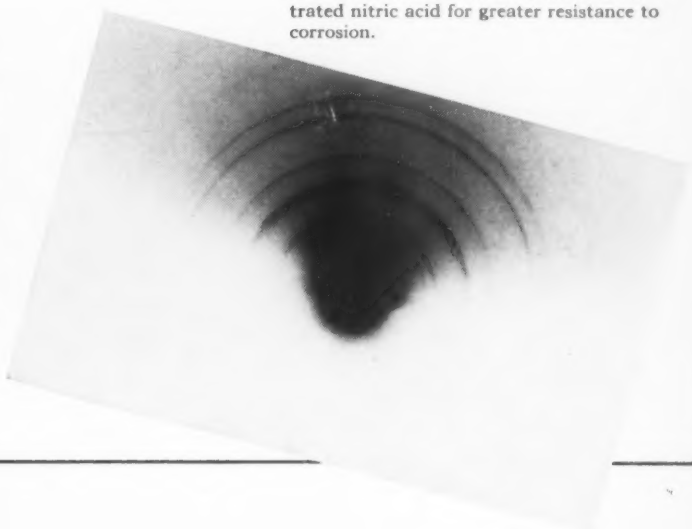
X-ray diffraction . . . To record crystalline composition, stresses, and thermal history by x-ray diffraction, the fastest film to use is *Kodak Industrial X-ray Film, Type K.* When smooth microdensitometer traces rather than highest film speed are required, use fine-grain *Kodak Industrial X-ray Film, Type A.*



This x-ray diffraction pattern shows, by the density variations along the Debye-Scherrer rings, notably by the patchy center, that the stainless steel used above is not isotropic in its properties.

This electron diffraction study (below) of the surface of the same stainless steel shows the presence of a thin layer, formed when the steel was treated with concentrated nitric acid for greater resistance to corrosion.

Electron diffraction . . . To record the nature and composition of thin films, layers, surface reaction products of materials by electron diffraction, Kodak offers the following products: *Kodak Medium Lantern Slide Plates,* and *Kodak Contrast Lantern Slide Plates,* and for the most critical work, *Kodak Spectroscopic Plates, Type 548-0.* Users also report that many highly informative electron diffraction patterns have been recorded on *Kodak Verichrome Film.*



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Kodak will send you free: a copy of the booklet "Materials for Spectrum Analysis" and a chart that tabulates Kodak films for x-ray diffraction. Your specific questions on Kodak Lantern Slide Plates for electron diffraction, and on other products mentioned above, will gladly be answered by correspondence.

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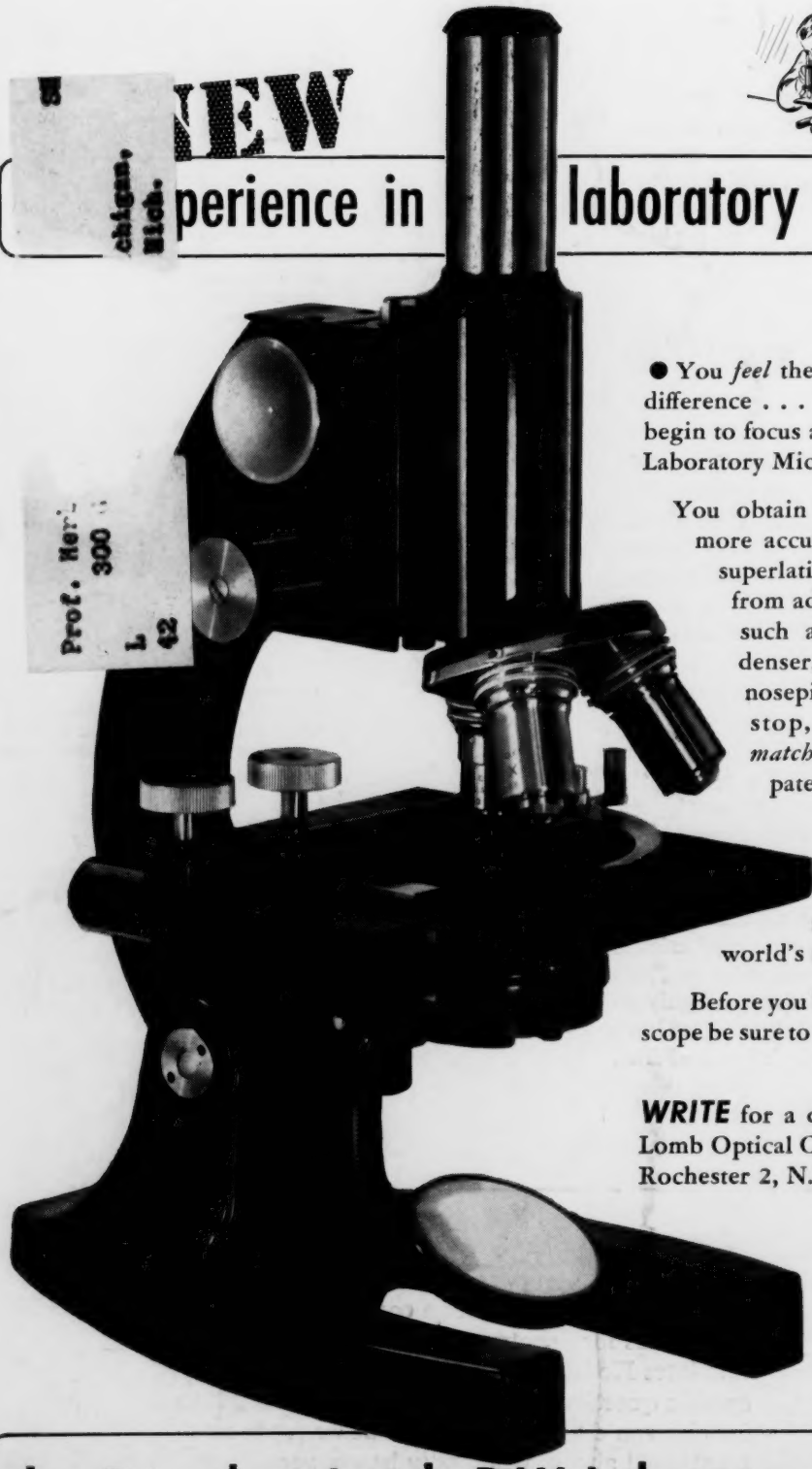

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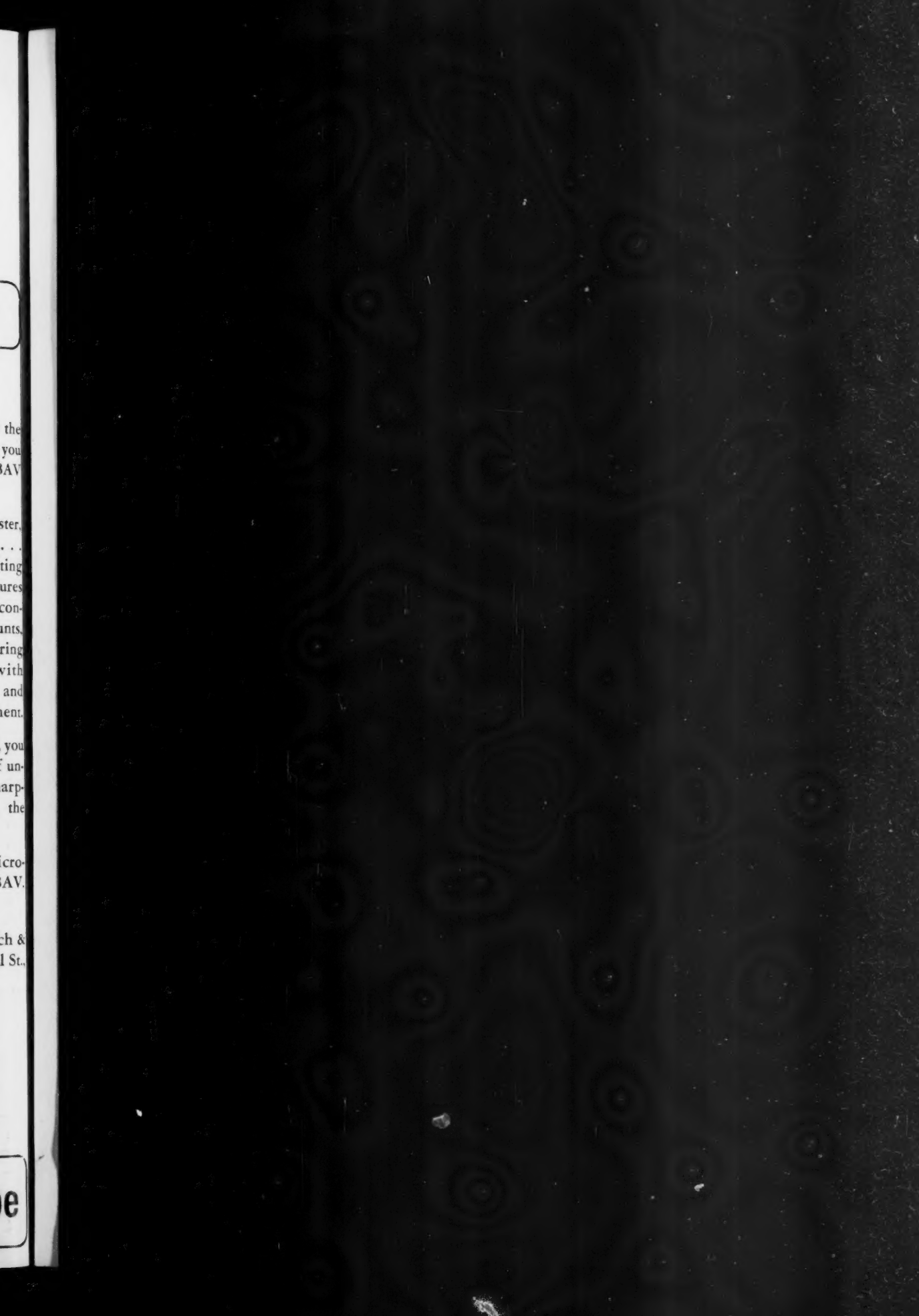
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